*PLURALISM IN ACTION: THEORIES AND PRACTICES OF FINANCIAL MARKETS*

**Pluralism in Action: Theories and Practices of Financial Markets**

**Pluralisme in actie:**
**theorieën en de praktijk van financiële markten**

**Thesis**

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*To my father*

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# Chapter 1

## Introduction and Aim of the Dissertation

The aim of this dissertation is twofold:

1) To add to a meaningful conversation on financial markets by using less conventional, yet empirical, methods, which differ from the dominant statistical empirical methods;
2) To provide a case study of an application of the less conventional methods, i.e. methodological pluralism.

While the various chapters may appear somewhat unrelated, the thread throughout is the interplay of theory and practice in financial markets.

Theory and practice have also played a constitutive role in the conception and production of the dissertation. It is the product of the author's experiences of the past thirty years in and around financial markets. More specifically, it has arisen from ten years of studying economics, finance and philosophy of science in academia, ten years of working experience in the sector itself (trading and asset management) and many years of policy-oriented research, mainly focused on the financial sector and the great financial crisis of 2007-2008. Combining these various levels of observation is somewhat unusual. I do not pretend to provide an exhaustive analysis on each level. Further elaboration on each of the levels is most probably possible, especially because both the practice of and academic thinking on financial markets have evolved and because I haven't always been part of the particular conversations on theory and practice.

## 1.1 WHY?

Financial markets are markets where financial products are traded: i.e. claims to assets such as stocks, bonds, currencies, derivatives, etc. Today's image of financial markets is one of big banks housed in skyscrapers, crowded exchange floors and yes, greed, crisis and turmoil. The perception has become negative because of the Great Financial Crisis in 2007-2008 and the ensuing recession. Yet such markets are a deeply ingrained part of modern society and serve a number of valuable functions: borrowing and lending, price determination, information aggregation and coordination, risk sharing, provision of liquidity and increasing (cost) efficiency by reducing transaction costs. The functions ultimately boil down to dealing with the problem of intertemporal consumption: how much to consume today and how much to save and/or invest for future use. The issue is relevant on all economic levels. An individual has to decide what to spend on items such as food or clothing today and how much to save for later, for instance for education or retirement. Corporations have to decide what to do with their profits: pay dividends to shareholders now or invest in order to create future profits. Governments face the budgeting question of how much taxes to raise and how to use the proceeds. Financial markets provide solutions to these various dilemmas by giving the

opportunity to transform the use of economic means with regard to purpose, location and time. Thus one can choose to buy insurance against certain events, borrow and lend money, consume, save and invest. Put differently, financial markets provide economic agents with a means to deal with risk, time, and uncertainty.

Financial markets are also very much a necessary institution to maintain and improve economic well-being on a macro-level. It would seem inconceivable for underdeveloped areas in the world to improve without, amongst other institutional characteristics, some form of financial means, for instance in the form of development aid, foreign investments and credit to local entrepreneurs (see Rajan and Zingales, 2003). Though financial markets affect pretty much anyone, even those who do not want to have anything to do with this trademark capitalist institution, the public at large commonly has taken their existence and functioning as a given. Only in times of crises when things that were taken for granted come under threat, do the markets make the headlines and get widespread public attention. That attention is negative most of the times because crisis breeds discontent. Thus financial markets are often regarded as a necessary evil, despite their importance and necessity. Their existence is continuously questioned, at least by important parts of the public.

Rajan and Zingales (2003) argue that competitive markets can be seen as a form of a public good. No one can be excluded from using them and use of them does not affect availability to others. But their existence and well-functioning are not a given. This applies to financial markets as well. Incentives and pressures are there that threaten their well-functioning. The threats arise from various sources. First of all there is a more or less natural process of competition. Market participants try to perform better than their competitors; if they do they may gain some form of market power. As less performing market participants are weeded out and if for some reason barriers to entry exist, successful market participants may develop a degree of monopoly power. Similarly, politicians and policy makers may have incentives to reduce competitiveness in a market, for instance for electoral purposes or in serving special interests instead of the common good. Attention needs to be paid to market structure because financial markets need to be liquid and transparent to function properly. This is where policy makers and regulators come in. Because, even while the very real possibility of distortion of competitiveness by politics and policy makers exists, at the same time Rajan and Zingales argue: "markets cannot flourish without the very visible hand of the government, which is needed to set up and maintain the infrastructure". In other words, unregulated markets are by no means always preferable to regulated markets, and vice versa.

That implies that the provision of public goods may require some form of collective action and that applies to financial markets as well. Usually governments are counted on to take that collective action. However, some public goods can be regarded as international or

global public goods, for instance the global climate and levels of pollution. In these cases some form of supranational coordination is necessary to ensure efficacy. The same applies to financial markets, simply because these are ultimately global markets. The great financial crisis of 2008 has it made painfully clear that the well-functioning of financial markets is an international matter, not a national one.

Within economics a significant subfield that deals with financial markets only started to develop after World War II. Subsequently the subfield, called finance or financial economics, went down its own path. Meanwhile, within the wider realm of economics financial markets were seen as a complementary institution to the real economy, which they were originally. The opinion was broadly shared by economists of varying schools which in turn influenced economic policymakers and politicians. The view was that economic processes are primarily driven by what happens in the real economy; what happens in the financial sector is a consequence of events in the real economy. This view has been formalized in most economic planning models that policymakers use and thus became embedded in actual economic policy. However, the financial crisis of 2007-2008 and the resulting fallout for the real economy serve as prime evidence that the relationship between the real economy and the financial sector is way more complicated and that turbulence in the financial can have serious repercussions on the real economy.

Finance became in the 1970s a hugely successful academic discipline within economics in terms of publications, journals and prizes, numbers of faculties, staff and students, and funds directed towards the field. It became the dominant conversation with regard to financial markets. Fuelled by some major breakthroughs, however, the focus of finance increasingly shifted away from looking at the functions of financial markets and towards investigating how the markets functions. Their existence, the primary function and its place in the economy were also taken as a given by financial economists and so they committed the same mistake in decoupling financial markets from the broader real economy. The dominant conversation was thus only dealing with a part of the phenomenon, although an important part.

The academic discipline of finance did more than analysing and theorising. It did significantly impact the markets themselves, in shape, size and structure. Theoretical developments such as the development of pricing models and new instruments such as various derivative products profoundly altered the markets and the world. In combination with the advance of technology the result has been that the market for money and capital is the largest, most international and most globalized market on earth. Money and other financial products fly all over the world in staggering amounts, twenty-four hours a day, transcending countries and continents. That material transcendence and their sheer size have made financial mar-

kets in a sense ungraspable for the public, for policy makers and perhaps also academics. An institution which in essence was complementary to the real economy, has become an 800 lb. Gorilla, a wild and dangerous beast which is nevertheless crucial to our well-being.

So on the one hand financial markets would seem a somewhat hard to grasp phenomenon which nevertheless affects us all, yet on the other hand our knowledge about these markets is fragmented and the attention devoted to it outside of its specific academic and professional realm limited and often ill-aimed. I aim to connect some of these fragmented thoughts and to provide some focus on how we can look at various aspects of financial markets using different tools from the toolbox of economics and other sciences. The goal is broadening and improving the conversation about financial markets, beyond what mainstream financial economics already has to offer.

## 1.2 HOW?

Financial markets can be looked upon in various frames. Sheila Dow (2016) has argued that these various ways of framing financial markets provide an argument for a pluralist or multidisciplinary or interdisciplinary approach beyond what financial economics has to offer. Moreover, financial markets, like most economic phenomena, are essentially an open system (ibid.). The links with other systems and entities (the real economy, firms, individuals, governments, etc.) are numerous and the boundaries not clearly marked. An employee with a pension plan is part of the financial sphere because his retirement savings will be the proceeds of some investment decision, individual or collective depending on legislation and regulation. At the same time the pension plan forms part of the labour compensation package of the employee and the operational budgeting considerations of the employer.

All economic phenomena are to a large extent social phenomena, depending on a multitude of interactions between various agents. That implies that the phenomena are not easily captured by covering laws like the iron laws of nature. Rather there are causal mechanisms at work which may give to rise to tendencies which are not necessarily permanent or persistent and can be affected by other mechanisms and may evolve as context and environment change. Much of economics is an attempt to depict causal mechanics (rather than covering laws). The issue is than whether these mechanisms are correctly described or whether the targeted mechanisms suffice for explanation and understanding.

Besides the goal of an improved understanding of financial markets I offer a pluralistic (within economics) approach (and to some extent also multidisciplinary and interdisciplinary)[1]. Finance in fact has pluralist origins. It is by no means a simple offshoot of economics. When one looks at the background of the main characters involved, one finds quite a colorful variety: mathematics, physics, medicine, law, French, and indeed economics, to name some. Much of the ground-breaking work has been done outside economics faculties at business schools, and even outside of academia at think-tanks, consultancy firms, banks and investment firms. Theory and practice in finance have had striking proximity, which has been a catalyst in the rise to prominence of financial economics. I said that academic finance has had a profound impact on the financial markets. The opposite is also true: the markets provide researchers with input: an incredible amount of empirical material in the form of asset prices and other market data. When thinking and theorizing about financial markets it is very much worthwhile to look at the practice and the practitioners.

Various types of pluralism can be identified (Dow, 1997) so some elaboration is needed on what is meant here, where pluralism applies and where not. Earlier in this chapter the functions of financial markets at the most basic level were set out: transformation of economic means for several different purposes, locations and times. Financial markets, even when being an open system, have emerged, exist and persist and they do so for a reason. In that sense the analysis here is ontologically monist, not pluralist. For example, as will be discussed in chapter five, derivatives markets do not exist because of the discovery of how to price derivatives but because they provide an economically efficient means to deal with risk, time and uncertainty.

However, monism on the ontological level does not imply that the ways to gain knowledge about financial markets, the methodological perspective used, and the specific methods to analyze are restricted. Put somewhat differently, the idea is that there can be many theories relating to one phenomenon (Mäki, 1997), different methodologies may add value (Dow, 1997) and diverse methods may shed light on varying aspects of the phenomenon (Groenewegen and Vromen, 1996).

John Davis (2019a) makes a useful distinction in this regard between methodological assumptions and substantive assumptions. Ontological claims involve substantive assumptions, he argues, and these assumptions involve ideological differences between researchers. He adds that these "ideological" differences between researchers may be considered irreconcilable"(or may be perceived as such), whereas conciliation is possible in regard to methodological assumptions. It is the latter type of conciliation that is mainly explored here.

1 Given opaque boundaries of a discipline pluralism within a discipline may or may not be a form of multi- and/or interdisciplinarity.

However, conciliation on the methodological level may also hint at the possibility of some reconciliation on the ontological level where it concerns substantive assumptions. Perhaps, at least in finance, the differences and disagreements between various economic schools of thought and other disciplines actually do not run as deep as they are often portrayed in a polarized academic arena.

Epistemological pluralism thus concerns methodological assumptions and is a matter of what different schools of thought (inside or outside of economics) have to say about financial markets. The idea is that different economic schools of thought provide meaningful insights, be it in terms of knowledge, justification, truth, adequacy, etc. With regard to financial markets the dominant paradigm is financial economics, in particular what has been labelled by Ross (2005) as neoclassical finance. Its key claim is that financial markets, if properly set up, will tend to efficient outcomes. Behavioral finance has become the main challenger to this paradigm. Because agents act less than fully rational in their decision making, deviations from efficiency will arise and persist and suboptimalities at the collective level can and do happen. The dispute between these two seemingly competing paradigms is further explored in chapter three. While these theories may appear rival they need not be in that different questions may be concerned which may relate to different subjects. The idea that behavioral and neoclassical finance are not rival but to a large extent complementary, is further explored in chapter four where another school of thought, Austrian economics, is used to bridge the apparent gap between neoclassical and behavioral finance.

Chapter four is an example of methodological pluralism: the use of different methodologies in relation to a phenomenon. Neoclassical and behavioral finance to a large extent share the same methodology[2]. Theories are presented as formal models. The models are then (often statistically) tested by examining hard data which leads to a verdict of either right or wrong. The Austrian School does share the tenet of methodological individualism with both neoclassical and behavioral economics but does have a fundamental distrust of quantitative methods and formal modelling as a means of capturing human action. The case is made in chapter four that their descriptive methodology can complement narrowly empiricist approaches.

2 Dow (2016) uses the label "logical positivism" to characterize this methodology. Whether that is a fortunate characterization can be debated, given the extensive philosophical discussions of the past on the subject of logical positivism (see for instance McCloskey, 1985). Dow identifies three key elements of what she labels logical positivism in economics. Besides empirical testing and the right/wrong demarcation she mentions the axiom of rational behavior. Following Ross (2005) and others, I do not subscribe to this in the case of finance. As Ross mentions, finance has taken a step back from this axiom in that in a well-structured and functioning market efficient outcomes come about by the arbitrage mechanism: a few rational acting agents suffice. See also chapter four.

That different methodologies can complement each other in that they relate to different aspects of a phenomenon does not say anything about the use of different methods for one and the same aspect of the phenomenon. Pluralism in method may actually be always be present. McCloskey (1990) has argued that while economists may extensively use the formal method of mathematics and modelling, in reality they are telling a story. McCloskey's point that economists should be aware of, and pay attention to, their narratives, is taken up throughout the following chapters. Statistical significance and neat regressions are not the only "facts" that matter. Financial markets lend themselves particularly well to this kind of testing: many of the processes are highly visible and financial markets produce an unrivalled amount of numerical data. The narrow empirical approach may however be well served by adding qualitative analysis of possible underlying mechanisms and institutional arrangements, and illustration by case studies and real-world examples[3]. That becomes even more important if indeed, as many hold (see for instance Bernstein, 1992, MacKenzie, 2005, Soros, 2013), theory and practice massively influence each other with regard to financial markets. In connection to this, it is interesting to observe what scientists actually do, in their scientific work but also beyond when they try to put their theories to the test in the market or in the arena of policy-making.

The argumentation in the analysis presented here makes use of quite different methods. A large part consists of historical data sampling and statistical investment analysis. As such, these can be regarded as a contribution to the quantitative turn in economic methodology and the history of economics (see for instance Düppe & Weintraub, 2018, Edwards, Giraud & Schinckus, 2018 and Cherrier & Svorenčík, 2018). But descriptive accounts and philosophical reflection are also employed.. The common denominator here is that all analysis is empirical or makes use of empirical observations[4]. Besides hard data, observations from the practice of the financial markets are used[5]. With regard to the realm of academics it is observed what scientists actually do, in their scientific work but also beyond, when they try to put their theories to the test in the market[6]. The interplay between theory and practice plays an important role throughout this dissertation. Many advocates of theoretical pluralism, me included, greatly stress that pluralism does not imply that "anything goes", to use the famous phrase of Paul Feyerabend (1975). However, with regard to pluralism in

3 Colander (2000) has argued that the label "neoclassical economics" in fact covers the descriptive, institutional brand of economics from the 1940s and the more formal modelling approach from the 1950s. A similar move, which is described in chapter two, can be found in finance.

4 Most of the research in modern financial economics is empirical but usually restricted to statistical data analysis.

5 These observations include experiences from the author's ten year career as a trader and manager in the financial markets.

6 Inspiration in this regard has been drawn from the field of social studies of science.

method there is no reason why certain methods or ideas should be excluded a priori as long as a serious confrontation with reality can be conducted.

## 1.3 WHAT?

In chapter two an extensive historical analysis is presented of the dominant discourse about financial markets, that of finance. The diversion between economics and finance is discussed and the development of finance through time, away from a more general macro-oriented perspective towards analysis of firms, markets and specific assets on the micro level, is displayed. Epistemically and methodologically the impact of the ground-breaking theoretical advances in the 1960s and 1970s becomes clear, in particular in the domains of asset pricing, efficient markets and agency theory. The lack of similar novel ideas since the 1970s combined with technological advances which have massively enlarged the possibilities for working with data, have resulted in predominantly quantitative empirical work.

That is not say that thinking about financial markets has gone stale. On the contrary: the breakthrough of behavioral economics, based on insights from psychology, has been largely fuelled by research on and data from financial markets. Chapter three explores the divide between the traditional neoclassical view that financial markets are to a large extent efficient and this most prominent challenger to that paradigm: the behavioral view that there are persistent deviations from efficiency because agents act less than optimally rational. It does so by looking at a very specific data set: the results of professional money management operations with which leading neoclassical and behavioral finance scholars are associated. The data do not provide crystal clear winners in terms of risk and return. In addition, there is no trace of exceptional performance when top academics are involved.

In chapter four it is argued that neoclassical and behavioral insights can be reconciled to a large extent by means of the Austrian theory of the market process (Kirzner, 1992). At the core, the behavioral claim of less than optimal rationality pertains to individual agents, while the neoclassical claim of efficient markets pertains to outcomes of interactions between many agents. The question then becomes if and how these interactions of less than perfect agents result in efficient outcomes. In the neoclassical account it is assumed that arbitrage —quick elimination of opportunities for excess profits—will ensure market efficiency. How realistic is that assumption? By looking at arbitrage as a dynamic market process where entrepreneurial discovery and learning take place content is given to that assumption. The upshot is that it may be more appropriate to talk about markets tending towards equilibrium, allowing for deviations from market efficiency which in time will be corrected. This could explain why

asset prices often display more volatility than is logical from an efficient markets standpoint, an important observation made by behavioral finance (Shiller, 2003).

The Austrian account, by focusing on "entrepreneurial skills" does not assume a fully rational homo economicus, rather a rational but not omniscient operator. The entrepreneur looks for opportunities and explores those. He or she may err at times, perhaps learn from mistakes and will adapt his or her behavior. Thus outcomes, prices, markets can be inefficient but these will correct at some point and ultimately will find their way back in the "right" direction. Radical uncertainty (Knight, 1921) plays an important role in this account. For if we acknowledge that economic processes are inherently uncertain, the outcomes and developments of such processes cannot be completely anticipated by calculation or precise estimation. Behavior of agents will be a matter of best-guessing, trial-and-error and opportunism. The market is nothing more than a device which brings together these imperfect agents with different mindsets and opinions to come to a collective outcome.

There are other scientific disciplines than (financial) economics which have paid attention to financial markets. Originating in sociological and philosophical circles in the 1990s the social studies of finance have sprung up. The aim is multidisciplinary application of social science disciplines such as sociology, anthropology, human geography, gender studies, socio-legal studies, and science and technology studies to the study of financial markets (Preda, 2007). One of the most interesting results has been the so-called performativity thesis: the idea that theory can enact the reality that theory aims to describe (Callon, 1998) Stretched to the limit this results in the provocative claim that theories can enact reality even when the theory is lacking in some form.

In chapter two the powerful influence of a few breakthrough theoretical advances for thinking about financial markets is shown, This influence went way beyond the academic realm: theories profoundly changed what actually happened in financial markets, in some cases actually creating new practices. Option pricing theory and the congruent development of derivatives markets is a particular noteworthy example in this regard; one that has been used to illustrate "performativity"[7]. Chapter five examines these performative properties of option pricing theory. It is concluded that option pricing theory indeed has had a profound effect on financial markets, because people actually started using the theory in practice. But it is not the case that this happened even when the theory itself was defective. Here once again the importance becomes apparent of distinguishing between what a theory does and does not claim, as well as identifying the accompanying assumptions and how realistic these assumptions are. On the other hand the nature of economic reality should be kept in mind.

7 Cf. MacKenzie, 2006a.

Economic phenomena are social phenomena which are subject to complex interactions in a wide and changing context instead of following some iron law of nature. Such phenomena are not easily captured in one grand theory of everything.

Having said that, I argue, in particular in chapter four with regard to the behavioral-neoclassical dispute that apparently rival theories may turn out not to be rival at all but rather complimentary to one another. The behavioral camp makes its claims largely based on psychology and experiments. The neoclassical camp making equally credible claims about collective outcomes on the level of the market where many agents interact, largely based on micro-economic concepts. The point of departure of both schools of thought thus differs: behavior of individuals versus collective outcomes. In other words, behavioral and neoclassical finance use different mechanisms of explanation (explanans).

Regarding the object to which the explanans is applicable (explanandum), the solid claims of behavioral finance pertain above all to the level of individual agents/persons. From there, it is inferred that collective market outcomes can be less than efficient. On the other hand, neoclassical finance postulates, given certain assumptions that collective outcomes should be efficient because of the no-arbitrage theorem .

The question then arises how the different levels are related: which mechanism links the level of the individual economic agent to collective outcomes in financial markets? In the case of neoclassical and behavioral finance an inter-theoretic bridge is suggested, inspired by the originally Austrian market process account, which gives actual content to the principle of arbitrage.

Since 2007 financial markets and the thinking about financial markets have profoundly changed with the events that will be labelled here as the great financial crisis. The crisis would appear to provide a harsh clash between theory and practice of financial markets. Some of the issues for finance and economics that have emerged from the crisis are examined in chapter six. The issues are numerous and quite different in scope and content. The multiplicity and diversity makes an argument for a broader, more comprehensive way of thinking about financial markets. Chapter six contains a proposal for a broader, enriched conversation on financial markets, a new institutional finance. The basic idea is that a plurality of partial accounts can accommodate a variety of claims and thoughts about financial markets on various levels of aggregation and identify interactions between varying claims. Because the need for such a broader and deeper, simply better understanding of financial markets has become painfully clear..

# Chapter 2

## Ketchup Economics: The Methodology of Finance

*This chapter contains an empirical investigation of the methodology of finance. An extensive sample, covering the entire history of the two leading journals in the field, the Journal of Finance and the Journal of Financial Economics, has been investigated in order to sketch 1) the development of subjects of research and 2) the approach used in tackling those subjects.*

## 2.1 INTRODUCTION

Back in 1985 Larry Summers employed the metaphor of "ketchup economics" to illustrate the relationship and differences between economics and finance. He distinguished two groups of researchers: general economists who study the ketchup market as part of the broader economic system and so-called "ketchup economists", located in the Department of Ketchup where they receive much higher salaries than do general economists[8]. "General economists focus on fundamental determinants of price and quantity of ketchup, the various supply and demand factors, and try to explain price fluctuations by examining various types of data and using models. Ketchup economists, on the other hand, reject this approach and its results. They point out that the aggregate data, used by general economists, are almost meaningless accounting entities which are not even accurately measurable in the first place. Instead they focus on studying the hard observable data of ketchup transaction prices and possible excess opportunities in the market. The lack thereof and the resulting efficiency of the ketchup market is regarded as the best established fact in empirical economics by ketchup economists" (ibid.)[9]. Translated, Summers states that general economists tend to focus on fundamental determinants of price and quantity, that is the various supply- and demand factors such as costs, wages, substitutes, income, etc., General economists do this in an attempt to explain price fluctuations, and they do so with mixed results. Financial economists reject the approach since it is based on useless accounting information and fraught with measurement problems. Instead they focus on observable transaction data. Financial economists are interested in the interrelationships of various prices and the existence of possible excess profit opportunities between those prices[10].

Others have made comments about the relationship and differences between economics and finance (Gibbons, 1987; Ross, 1987 & 2005; Campbell, 1994, Harrison 1997, Jovanovic 2008 & 2012). Peter Bernstein (1992, 2007) argued from a historical perspective that finance has changed through the years from a descriptive, qualitative endeavor into a formalized quantitative one. This move, he claims, was fueled by theoretical innovation[11]. The stories and comments mentioned above are to a large extent a claim about methodology: how the same phenomenon can be approached from various angles and with different methods.

8 Note that Summers' remarks date from a period before the Nobel Prize in economics was to be awarded to a finance scholar. The first would be Harry Markowitz, William Sharpe and Merton Miller in 1990, followed by Robert Merton and Myron Scholes in 1997, Eugene Fama, Lars Peter Hansen and Robert Shiller in 2013 and Richard Thaler in 2017.

9 Summers's remarks date back to the time period before the rise to prominence of behavioral economics and finance; a development in which Summers himself played a significant role.

10 Summers seems to focus mainly on asset pricing here. Of course finance is comprised of more subjects such as corporate finance and governance and banking.

11 See also Jovanovic 2008, 2012 & 2018.

But, given that there might be methodological differences between finance and economics in general, while a large literature exists today on the methodology and philosophy of general economics, there is not really such a thing in finance[12]. Besides the scattered remarks above, there are papers and chapters on method and ways of doing research. These include the viability of certain statistical procedures, the proper use of data, and how (un)realistic certain assumptions are, but these are usually confined to a technical treatment (see for instance Cochrane, 2001, Harvey, 2017). Which least squares calculation is most appropriate? What distribution of returns fits best[13]? How are data collected and used?

Some literature exists on what has had impact in finance in terms of specific papers, specific scholars, and specific academic institutions (see Arnold et al., 2003, Keloharju, 2008). The research is based on citations and is usually limited to a particular time frame: Arnold et al. (2003) cover the 1990s while Keloharju (2008) investigates the new millennium. Kim, Morse, and Zingales (2006) have performed a similar study on economics at large, covering the period from 1970 till 2000.[14]

There would appear to be space for a thorough, fundamental treatment of the methodology of finance, which includes but is not limited to its connections with economics at large.[15] A first step would be to investigate the ways of argumentation in finance. A sample from the entire history of editions of the Journal of Finance and the Journal of Financial Economics, the two leading journals in the field, has been examined in order to answer two basic questions. First, what are people writing about, and, second how do they write about it? In other words, what have been the subjects of the papers and what approach is used in tackling these various subjects: empirical, theoretical, or a mixture of both?

The aim is to empirically check the scattered notions that have been sketched above and gain insight from the bottom up in the ways of argumentation in finance. A longitudinal perspective is taken in order to track the developments through time, since scientific fields are hardly ever static and thus methodological remarks are bound to be context-sensitive. An attempt will be also made to connect the findings to familiar concepts in the methodology, philosophy, rhetoric, and history of economics.

12 For instance Reiss's 2013 textbook "Philosophy of Economics: a Contemporary Introduction" is an accessible and worthwhile example.

13 In the words of McCloskey: "small-m methodology".

14 Edwards, Giraud and Schinckus (2018) argue that similar attempts have had a long tradition since the 1960s.

15 De Scheemaekere (2009) has published a paper titled "The Epistemology of Modern Finance", which would suggest such an attempt. While interesting in many regards, De Scheemaekere's analysis is limited to the presence and use of mathematical models in finance. It will be shown that mathematical modelling is only a part of finance research.

The method of analysis employed in this chapter is inspired by the works of Deirdre McCloskey and Arjo Klamer. Klamer (2006) has described economics as "a bunch of conversations". This chapter can be seen as an attempt to operationalize that notion in that it tries to map one of those conversations: finance. There are also similarities with McCloskey's seminal work on the rhetoric of economics (1986, 1998) and on the use of statistics (1996, 2008), coauthored with Ziliak. While the analysis in this chapter is simpler and more superficial than McCloskey's, it does have in common that what is considered, is what particular scholars actually do, not what they should do. Besides minimizing the normative bite, there is also no epistemic appraisal or deep reading in the "quick and dirty" approach used here. This may present advantages with regard to criticism of perceived subjectivism in interpretive studies. At the same time, I'm claiming that the bottom-up approach still brings out the grand plots and the crucial moves and twists, based on an empirical foundation.

## 2.2 VARIOUS PERSPECTIVES ON THE METHODOLOGY OF FINANCE

Let's start by examining the claims about the methodology of finance in a bit more detail. Summers (1985) starts out with the observation that, while economics and finance are clearly allied and the latter undoubtedly has its roots in the former, increasingly two different cultures and unconnected literatures have emerged, even when the same issues are addressed. He claims that general economists ask the right questions but that they lack adequate data, theory, and empirical methods. Financial economists forego these more important questions on the fundamentals of asset prices in general, by exclusively focusing on hard pricing data[16]. In Summers' opinion: "The increasing disjunction of the fields of economics and finance are obviously inefficient" (ibid.). It would appear then that Summers' main point resides in a distinction in the level of detail in the analysis. The various differences in data, theory, and method can then regarded as a result of that difference in focal points.

Stephen Ross (1987) was in agreement with Summers that important differences have surfaced between finance and economics, despite the apparent interrelations, but he insists that there is nothing wrong with that. He pinpoints the distinction as essentially a methodological one. His considerations are the following, many of them not unlike those of Summers. First, in finance data are huge in quantity and of high quality. Second, "there is a strong and subtle pressure to build models that utilize the data within the financial database" (ibid.) leading to a focus on relative pricing based on risk-return characteristics. Third, the bulk of the data are price data rather than volume data: "finance theory is a theory of inelastic supply, and of price determination". That may sound pragmatic but it is not the whole story.

16 Again Summers seems to limit his observations to asset pricing while foregoing others parts of financial economics.

For the purpose of price determination financial markets are assumed as good as perfectly competitive with unlimited liquidity available. Whether that is always the case in practice, and to what extent, can be debated, but it is true that money and capital fly over the world instantaneously twenty-four hours a day. Economics, according to Ross, is characterized by "the apparatus of demand and supply and the attendant notions of equilibrium" and that also applies to game theory. In contrast, "the focus of finance is micro theoretic and the intuition of finance is the absence of arbitrage". Arbitrage can be defined as the possibility of simultaneous buying and selling of goods (securities, currency, commodities, etc.) in different markets or in derivative forms in order to take advantage of differing prices for the same asset[17].

Demand curves in financial markets are horizontal because of the plethora of substitutes; supply curves are either perfectly elastic or inelastic depending on the situation (i.e. investing or financing). Their interplay is therefore only meaningful at the highest aggregate level and not in the analysis of specific assets and markets. Finance is characterized by the simple intuition that information is reflected in prices and that arbitrage opportunities are short-lived. Those intuitions have brought the field to great heights. According to Ross, different focus will give different insights, for example on a macro level, or on the level of agent behavior. Although Summers and Ross do not agree on the desirability of a disjunction between economics and finance there are some striking similarities in their assessments, in particular the top-down perspective used in economics versus the bottom-up approach used in finance.

Michael Gibbons (1987) provides an empirical perspective to the interrelations of economics and finance. As a starting point, for Gibbons finance is a field within economics that has borrowed from other fields in economics just as the rest of economics has borrowed from finance. The sharing of econometric methods is one case of such two-way traffic. Another is a common interest in certain asset prices, such as bond prices and interest rates. Here the difference in focus shows up again: macroeconomists are interested in policy issues and effects on the real economy while financial economists are interested in the market picture as a whole and price determination. Finance also provides an excellent laboratory for a number of other fields within economics through its huge database. Examples are economics of regulation and economics of information. Financial data are also frequently used for the measurement of unobservables, such as expectations about inflation, interest rates and earnings, which are of importance to economics and the economy in general[18]. And finally there are the empirical anomalies in finance which cast doubt on rational behavior and efficient

17 The notion of absence of arbitrage can be traced back to the Marshallian tradition in economics.

18 A famous example is the market in frozen orange juice. The expectations about the weather that are implicit to the prices have proven to outperform the forecasts of meteorologists (Ross, 2005).

markets. A famous example of these observed deviations from theoretical benchmarks is the so-called January effect: evidence that stock prices rise disproportionally in January (Thaler, 1987). Much of the impetus that behavioral economics has enjoyed has been fed by finance data.

Of more recent date are John Campbell's remarks about the New Palgrave Dictionary of Money and Finance (1994). His starting point is interesting in this context: why a separate Palgrave for money and finance besides the familiar one on economics, which dates back to the 1890s? Obviously there are commercial reasons given the size and importance of the financial services industry. But the question remains whether financial (and monetary) economics are somehow different from economics in general. Campbell points at the history of finance, developing from an obscure, lowly regarded subfield of postwar economics into a prominent, highly visible field with its fair share of Nobel accolades. Despite the contributions of "general" economists (for example Modigliani, Tobin, and Samuelson) modern finance has developed itself somewhat independently of the rest of economics. Campbell cites the distinct literatures on rational expectations in macroeconomics and finance as a case in point. Ross' arbitrage argument plays an important role in the distinctness of the two as well. But, like Gibbons, Campbell also argues that finance has been particularly successful in employing broader economic concepts: equilibrium theorizing in theoretical asset pricing, econometrics in empirical asset pricing, and game theory, agency theory, and information economics in corporate finance. As such, there is enough coherence and substance in financial economics to justify a separate standing (and thus a separate Palgrave).

Peter Bernstein has authored a couple of books (1992, 2007) about the subject. In his 1992 book he describes the coming about of the main theories in academic finance, what he calls the "Capital Ideas", and the enormous impact they had on the practice of financial markets[19]. Bernstein told the story of finance, growing from a descriptive, institutional discipline into a formalistic, quantitative one fueled by the breakthrough contributions provided by Markowitz, Tobin, Modigliani, Miller, Samuelson Sharpe, Fama, Black, Scholes, Merton, and others. In his 2007 follow-up book, Bernstein argued that these "Capital Ideas" still form the heart and soul of finance. Current research in finance is still mostly concerned with some form of application of those core theories.

Merton Miller has reflected (1999a, 1999b) on the history of finance as well. He sees a difference in perspective between finance and economics. Finance has a "micro-normative"

19 The latter is in itself interesting since it relates to the performativity issue of financial economics, as most extensively treated by Donald MacKenzie (2006). This issue is extensively treated in chapter 6, containing a critical appraisal of MacKenzie's analysis and argumentation.

approach, which he traces back to the business school roots which finance has[20]. Economics, on the other hand, uses a "macro-normative" approach. Miller also confirms the change that Bernstein described from descriptive and institutional to formalistic and quantitative. He adds that "the typical paper in the Journal of Finance consists of two sections: the first presenting the model, the second an empirical part with real-world data which are usually consistent with the model" (which, in his opinion, is not surprising because had that not been the case, the author would not have submitted the paper in the first place, and the editors would never have accepted the article for publication!) (ibid.). He later adds that "the profession, from the outset, wholeheartedly adopted the Friedman positivist view: that what counts is not the literal accuracy of the assumptions, but the predictions of the model" (ibid.). This Friedman positivist view (see Friedman, 1953) translates in a primary concern with testable hypotheses (see for example Fama, 1998). However, the same is true for economics, in his opinion.

The claim that finance is characterized by positivism is also made by Sheila Dow (2016) although she may not mean the exact same thing with that label[21]. Predictions rather exist than explanations, empirical testing against "facts", and a formal mathematical representation can be considered as the staple marks of this methodology (ibid.). She adds that behavioral finance is no different in this regard than neoclassical finance.

The point of departure for Paul Harrison (1997) is economics in general. He argues that arbitrage was the crucial concept that allowed economics to revolutionize finance. But it also ensured that finance became prominent within economics (and at the same time legitimized financial markets both in society and as an interesting research subject). Finance provided economics with rigorous methods and hard empirical research and thus became one of the pin-up girls of the neoclassical paradigm.

Based on the above there appears to be some general agreement about what finance is about and in what sense it differs from economics in general:

1) Finance focuses on the micro-level analysis of financial markets in a broad sense: it is mostly concerned with markets and firms, not with aggregate entities or individual behavior. Economics, on the contrary, uses more of a macro approach, even when the same subjects are analyzed, such as a particular financial market, or the same concepts are used, for example rational expectations.

20 Miller himself was at the University of Chicago's Graduate Business School.

21 Friedman's 1953 paper "The Methodology of Positive Economics", popularly known as F53, has been and probably still is the most hotly debated paper ever written on economic methodology. Many varying interpretations exist; no general agreement is present. See Mäki, 2009, for elaborate discussions of F53.

2) Finance can be characterized as a thoroughly positivist discipline in the sense that testing of hypotheses and meaningful predictions matter; rigorous use of data is basic and theory follows. In comparison to economics, finance is more empirically inclined.
3) Finance, like economics, has changed and evolved over time, in particular fueled by a handful of major breakthrough contributions. Other methods and approaches have been the result.

## 2.3 RESEARCH DESIGN

The Journal of Finance (JoF) is published by the American Finance Association (AFA), which describes itself as "the premier academic organization devoted to the study and promotion of knowledge about financial economics". First published in 1946, the JoF has grown into one of the most prominent journals in the field of business, finance, and economics. In terms of impact factor, it has been ranked consistently in the top ten of any ranking of economics journals, both in impact and number of citations. It is by far the highest ranked specialized journal. Nowadays six issues per year appear, growing from three editions in its first year of publishing.

The sample consists of the entire history of the journal, starting with volume one, issue one, form August 1946 running up to volume 72, issue four, August 2017. From each year one issue has been surveyed, starting with volume one, issue one from 1946. Next is volume two, issue two (1947), then volume three, issue three (1948), etc. There are two reasons for using this procedure. First, in this way the papers from the annual meeting of the AFA, which appear in a separate issue, are also included in the sample. Since those meetings are an important outlet for finance research, it was considered valuable to include these special issues. Second, to avoid "seasonal" effects: frequently papers with a similar subject are being published in one and the same edition. In total seventy-two years were covered, comprising 873 papers.

In order to categorize the papers according to subject, the JoF's own categorization, which corresponds to the familiar household Journal of Economic Literature (JEL) classification, has been followed as much as possible. Between 1978 and 1999 the JoF published the distribution of sent and accepted papers among subjects in the annual report of the editor[22].

22 This also provided a check for the representativeness of the sample; see paragraph 2.6.

To complicate matters, this classification has been changed on at least five occasions. The one from 1999[23] was used in the sample. The five main categories are:

- Global Financial Markets, which includes asset pricing, derivatives pricing, information and market efficiency, investor behavior and market microstructure;
- Corporate Finance & Governance, which includes capital budgeting and investment policy, financing policy, capital and ownership structure (incl. agency issues), financial distress, mergers and acquisitions, and dividend policy;
- Financial Institutions, which includes banking, insurance and other financial institutions and financial intermediation;
- Money and Interest Rates, which includes determination and term structure of interest rates, monetary policy and public finance and other macroeconomic and policy aspects of financial markets;
- Other, including academic institutions, academic publishing and academic education.

This categorization is of course not clear-cut. For example taxes and international finance can apply to more than one category. In the sample each paper has been assigned one or more tags after which it was assigned to one of the five categories. In the case of the first category, Global Financial Markets, the subdivision has also been analyzed, since there appeared some significant shifts there over time.

In order to categorize the papers according to approach, two questions were asked. First, is the central research question mainly theoretically or mainly empirically inspired? Second, are the arguments and evidence mainly theoretical or empirical? Based on the answers to these two questions, an article was either categorized as theoretical, empirical or a mixture of both. The last category comprises mainly papers that have a strong theoretical component in the research question and a strong empirical component in the nature of evidence. Well-known exemplars of purely theoretical papers are the ground-breaking papers of Sharpe (1964) which lays out the Capital Asset Pricing Model and Jensen & Meckling (1976) on agency theory.

Purely empirical are for instance many of the papers of Eugene Fama and Kenneth French on asset pricing (e.g. 1992, 1993). Examples of papers which were considered as belonging to the mixed category are Lee, Shleifer & Thaler (1991) and Fama (1998).

23 Newer versions of the JEL classification do exist in which Global Financial Markets is changed to General Financial Markets. Money and Interest Rates has been removed from the Financial Economics chapter and Behavioral Finance and Household Finance were added.

In categorizing observations were made on specific method: formalistic or not, quantitative or not, theorem proof, statistical evidence, anecdotal evidence, experimental evidence, simulation, case study, survey, questionnaire, interviews.

In order to examine if the results of the JoF analysis carry over to the whole field of finance a look has also been taken at the Journal of Financial Economics (JFE). The JFE is the second highest ranked finance journal and in itself quite a prominent publication, also ranking consistently in the top ten of any ranking of economics journals, both in impact and number of citations. The same research procedure has been applied: one issue from each calendar year is analyzed in descending order starting with the most recent issue. Note that the sample is much smaller: 313 papers in total as compared to 873 for the JoF. The reason is that the JFE has only been in existence since 1974 and it publishes more issues per year with in general less articles per issue. The results for the JFE are analyzed by themselves and in comparison with the JoF for the matching period, i.e. the period between 1974 and 2018.

Finally, it has to be emphasized that epistemic appraisal of the papers itself has no part whatsoever in this analysis. Besides the enormous amount of time required to do so for almost twelve-hundred papers, this paper is strictly confined to methodology.

## 2.4 EMPIRICAL RESULTS SUBJECT OF RESEARCH

Figure 1 shows the data from the JoF on subject, summarized over five year intervals, in ascending order starting in 1946, going forward up to 2018.

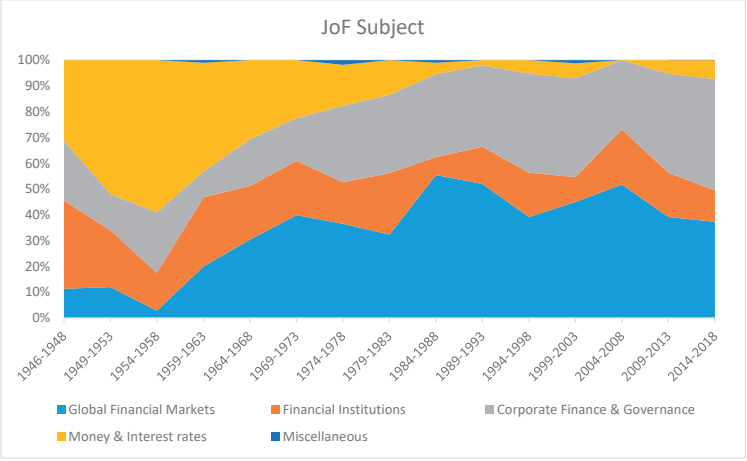


**Fig.1.** Journal of Finance subject 1946-2018

Most striking in the graph is the shift from typical subjects of general economics, money and interest rates, towards financial markets and corporate finance. Starting out on the left side of the graph, papers on money and interest rates gradually decline from over forty percent towards a level of around five percent in 1983 when it stabilizes. In that same time period, papers on corporate finance and on financial markets both grow from ten percent to the thirty percent range. After 1983, the share of corporate finance & governance stabilizes but the share of global financial markets increases even more towards the forty and fifty percent range. Contributions on financial institutions remain relatively stable, although in the first interval period its share was quite high. This could reflect attention for rebuilding a variety of institutions in the immediate post-war era.

What becomes clear from these data is the impact of the major theoretical breakthroughs in finance. It is not a stretch to assume that the work of Modigliani and Miller, dating back to the late fifties, and the work on agency theory by Jensen & Meckling (and Ross, 1973[24]), have spawned an enormous amount of new research and the accompanying publications on corporate finance & governance. In the middle 1970s there appears to be a significant impetus which may well be attributable to the impact of the Jensen & Meckling (1976) JFE paper. Likewise for the area of global financial markets: Markowitz's work on portfolio theory in the fifties, but especially the Capital Asset Pricing Model (CAPM) by Sharpe and others, and the efficient markets ideas by Fama and others, both in the sixties, followed by option pricing theory in the seventies have propelled this area of research to a dominant position.

24 There has been some debate about who actually pioneered agency theory. In economics Ross (1973) is often acknowledged as the first one to explicitly adress the principal-agent problem. In the same year political scientist Barry Mitnick (1973) published on the institutional theory of agency.

## Six key finance concepts

Modigliani-Miller Theorem: states that the market value of a company is calculated using its earning power and the risk of its underlying assets and is independent of the way it finances investments (equity or debt) or distributes dividends. The reason being that investors are diversified and make adjustments to accommodate for varying risk and return characteristics. Given certain assumptions (most notably efficient markets) the implications are twofold:

1) in the absence of taxes, bankruptcy costs, agency costs, and asymmetric information, and in an efficient market, the value of a firm is unaffected by how that firm is financed.
2) When taxes are present and interest on debt is tax-deductible, using debt actually increases the value of the company.

The "M&M" theorem is one of the cornerstones of capital structure and corporate finance theory.

Modern Portfolio Theory (MPT): a theory on how risk-averse investors can construct portfolios to optimize or maximize expected return based on a given level of market risk, emphasizing that risk is an inherent part of higher reward. Based on statistical measures such as variance and correlation, an individual investment's return is less important than how the investment behaves in the context of the entire portfolio. Put very simply, MPT embodies the idea that one shouldn't put all one's eggs in one basket.

Capital Asset Pricing Model: a model (commonly known as CAPM) that describes the relationship between the expected return and risk of investing in a security. It shows that the expected return on a security is equal to the risk-free return plus a risk premium, which is based on the asset beta of that security, i.e. the volatility of the security relative to the market as a whole. Put very simply, the model implies that in order to achieve higher expected returns on an asset, one should expect more risk. Market efficiency is a core assumption.

Efficient Market Hypothesis: the hypothesis that states that asset prices reflect all available information. A direct implication is that it should be impossible to outperform the market consistently and systematically on a risk-adjusted basis since market prices should only react to new information. Closely related is the random walk concept: market prices evolve according to a random walk (so price changes are random) and

thus cannot be predicted. In an efficient market prices should follow a random walk. However, while a random walk pattern can be seen as evidence for market efficiency, it is not a definite proof.

Agency Theory: a principle that is used to explain and resolve issues in the relationship between principals and their agents. For instance, the relationship between shareholders, as principals, and company executive, as agents. Principals delegate decision-making authority to agents. Because many decisions that affect the principal financially are made by the agent, differences of opinion and even differences in priorities and interests can arise, which can lead to conflicts of interest and may induce moral hazard. This is sometimes referred to as the principal-agent problem. Agency theory (also called Principal Agent Theory) is a core concept in the area of corporate governance, corporate finance and the theory of the firm.

Option Pricing Theory: the theory which enables theoretical valuation of options and other derivatives using various variables (underlying value, exercise price, volatility, interest rate, time to expiration, dividends, etc.). The breakthrough insight of option pricing theory is that the value of options and other derivatives is independent of risk and return characteristics of the underlying asset. Rather, the value of an option other derivative crucially depends on the volatility (standard deviation) of the underlying asset. Within Option Pricing Theory there are various models, which can be used for calculation of the value of a particular option or derivative, for instance the Black-Scholes model and the Cox-Ross-Rubinstein model (also known as the binomial model).

If one distinguishes within the category global financial markets between asset pricing and other subjects the impact of novel theory becomes even more clear: see figure II. "Other subjects" includes topics such as market structure, market analysis and investor behavior, including tests of market efficiency and rationality.

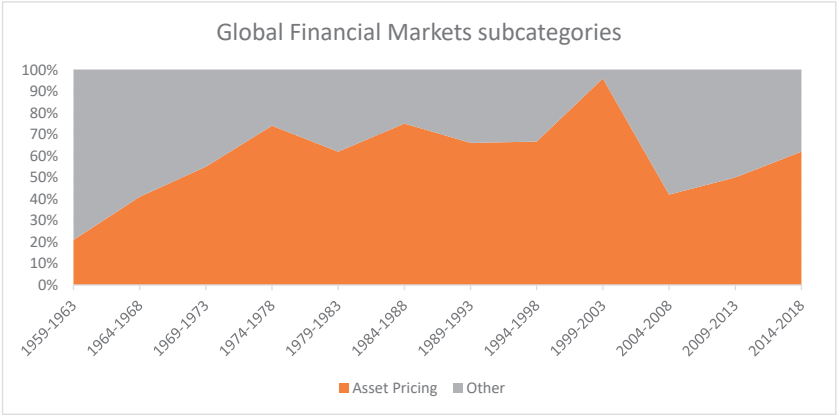


**Fig.2.** Journal of Finance subject 1959-2018[25]

From the 1960s onwards the share of papers on asset pricing rose steadily to approximately two-thirds of the total number of papers within the category global financial markets during the 1970s[25]. Fueled by CAPM, the variety of successors it has spawned, and numerous empirical tests on those models, the share of asset pricing contributions peaked during the turn of the century, followed by a noticeable drop-off. This drop-off coincides with the rise to prominence and recognition of behavioral finance. Most of the behavioral finance papers fall in the subcategories investor behavior and market analysis. A final observation can be made on a possible impact of the 2008 great financial crisis. The impact of the crisis on finance will be discussed more elaborately later on in this chapter, but it would appear that the crisis, amongst other things, has resulted in renewed interest in asset pricing. Given the fact bubbles in certain asset prices are seen as one of the causes of the crisis, that should not come as a surprise.

What about the Journal of Financial Economics? Figure 3 displays the graph for the JFE for its entire period of existence, i.e. from 1974 up until 2018.

25 For the sake of clarity, option pricing theory was included in the category of asset pricing . Developed in 1973, option pricing theory has been a highlight of theorizing in finance. For a relatively short period there has been considerable research interest in it which explains some of the spike in the share of asset pricing. Nowadays pure theory of valuation of options has become a specialized and highly quantitative and mathematical endeavor. That is probably why a relatively general journal such as the JoF doesn't carry many papers on the topic anymore.

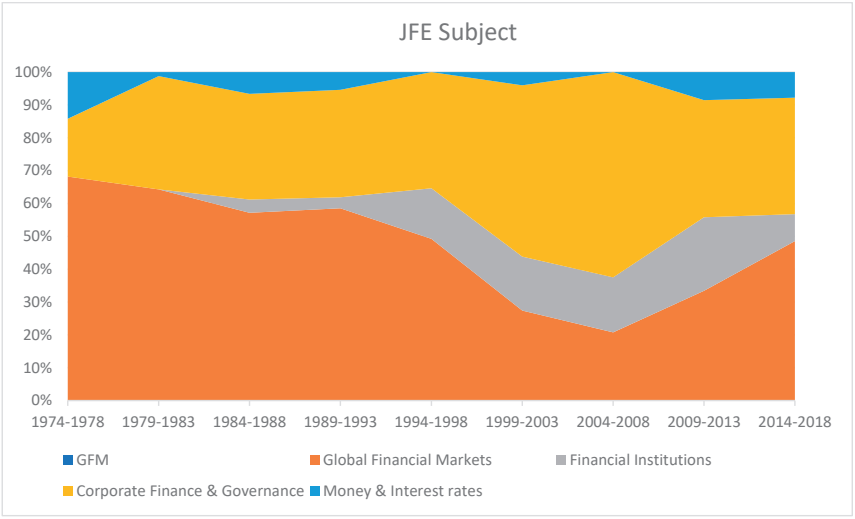


**Fig. 3.** Journal of Financial Economics subject 1974-2018

In order to compare the two journals, in figure 4 the graph for the JoF is shown for the corresponding period 1974-2018.

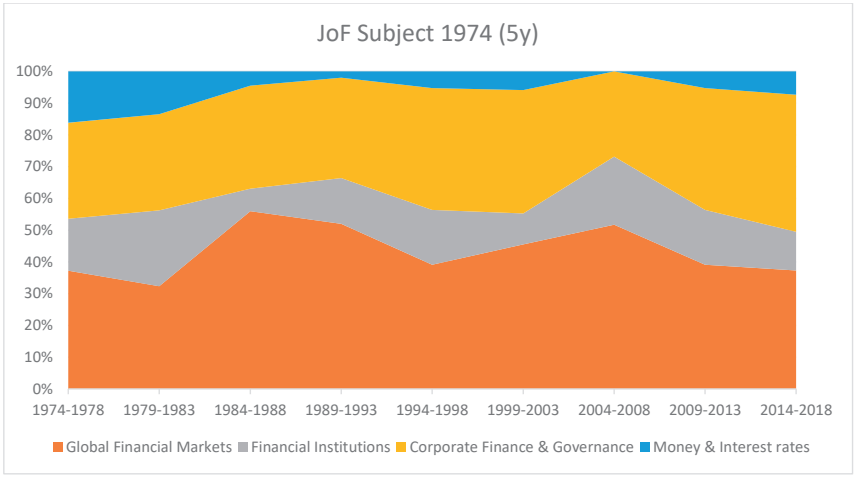


**Fig. 4.** Journal of Finance subject 1974-2018

In both journals the categories corporate finance & governance, and global financial markets are dominant. But whereas in the JoF the share of GFM steadily rises with time, initially it falls in the JFE at the expense of CFG. This might have been just a matter of division of territory: journal specializing in a certain area. It is probably also attributable to the longstanding

involvement of Michael Jensen with the JFE, having been one of the founders of the journal and its managing editor for close to twenty-five years. His work is mainly concerned with corporate finance and governance[26]. The share of papers on financial institutions is comparable, as is the minor share of entries on money and interest rates.

If we delve a bit deeper inside the categories for the JFE, once again asset pricing, market analysis, and corporate finance are the most prominent subcategories, displaying roughly the same tendencies through time as the JoF. What is different from the JoF, is the significant share in the more recent periods of papers on corporate governance. This is not surprising given the increased attention this topic has received in the form of public debates about the interests of various stakeholders, sustainable and responsible business, and compensation issues. The JFE, specialized on corporate finance and governance, is clearly the preeminent academic outlet for these topics.

Some observations can be made. First, the revolutionary developments in theory have clearly changed the field and shaped finance's somewhat distinct identity. The resulting increased attention for corporate finance and financial markets has come at the detriment of traditional economics. Moreover, the breadth of the range of subjects has changed dramatically as well. In the early days one could find papers on disarmament, post-war reconstruction, urban development, real estate, even philanthropy. Much of the work was explicitly relevant to economic policy making. The macro and welfare perspectives have given way to micro analysis. Nowadays it is about specific markets or asset categories, compensation, tax and dividend issues, or even narrower, about isolated phenomena such as the (in)famous anomalies. That is not to say that finance has removed itself further from economic reality. Rather it is concerned with a different, and perhaps narrower, part of economic reality. Academic finance has not been an isolated intellectual endeavor, but has clearly been shaped by the world out there, from thinking about the challenges for the financial system that the world faced after WW II, to the questions that society poses more recently, for instance with regard to governance.

The great financial crisis can be regarded as a gamechanger in the financial markets: an event which affected and altered thinking on financial markets as well as establishing and providing a set of new, unique data. Below is plotted how the division of subjects developed in three year intervals in the period 1997-2017, i.e. ten years before the crisis started and ten years onwards.

26 Though not exclusively; see for instance Jensen, 1978.

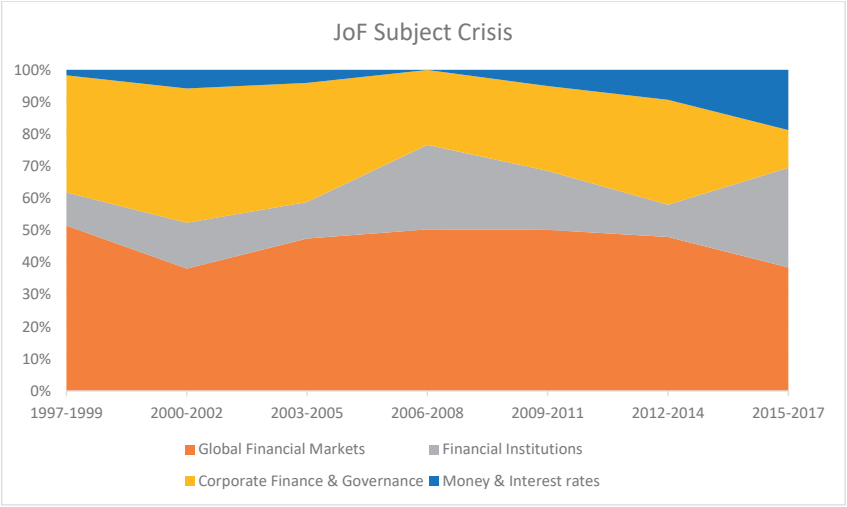


**Fig. 5.** Journal of Finance subject 1997-2017

The impact of the crisis appears to become somewhat visible in the JoF from 2011 onwards. Since then every journal from the analyzed sample contains crisis-related articles. Given that the average time between acceptance and publication for the JoF is almost twenty months (Holden, 2017), that's is not surprising. Regarding the subject categories some renewed interest in the categories FI and MI is visible since the crisis, mostly at the expense of CFG.

In the JFE crisis-related articles appear a year earlier, in 2010. That corresponds to the average time between acceptance and publication for the JFE of almost ten months (Holden, 2017). From 2010 onwards, a large majority of the journals from the analyzed sample contain crisis-related articles, the first one being a special issue entirely devoted to the crisis (Vol. 97:3 "The 2007-8 financial crisis: Lessons from corporate finance", dated September 2010).

In the JFE initially there is also some additional attention for the categories FI and MI, though less pronounced than in the JoF and subsiding recently. One could conjecture that the fallout of the financial crisis on the economy (and economics) at large has given rise to this renewed attention for the categories FI and MI, which relate more to economics in general than the two "supers-specialties" of Finance, being GFM and CFG.

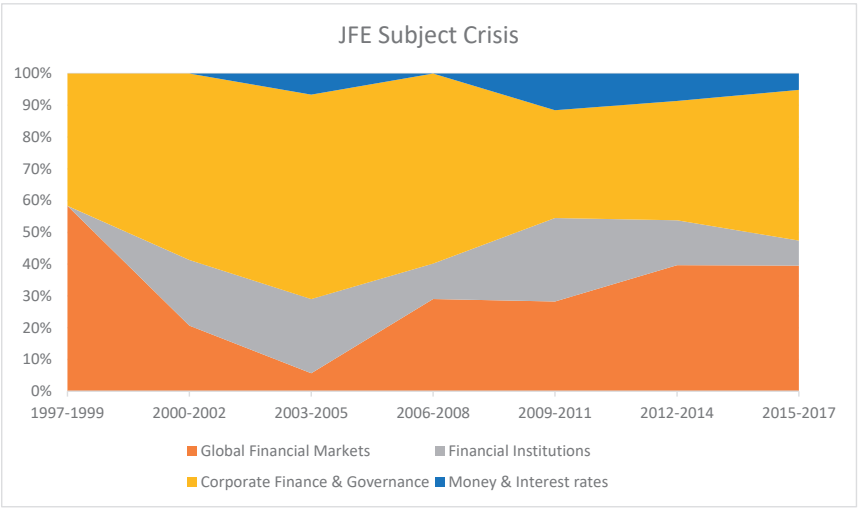


**Fig. 6.** Journal of Financial Economics subject 1997-2017

## 2.5 EMPIRICAL RESULTS RESEARCH APPROACH

Below in figure 7 are the data on the approach used towards the various subjects, again summarized over five years, in descending order starting in 2008 going backward to 1959.

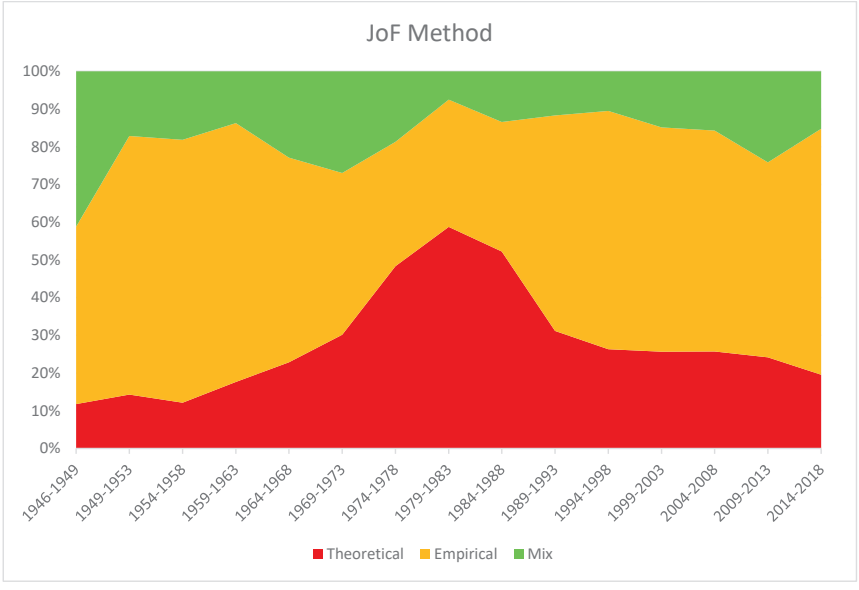


**Fig. 7.** Journal of Finance approach 1946-2018

Most striking in this graph is the rise in theoretical work in the late sixties, seventies up until the middle eighties. The early majority of empirical inquiry gives way to a majority of theoretical research, only to become dominant again at the end of the 1980s. Once again the consequences of the major theoretical breakthroughs can be inferred. In particular the formal, mathematical approach, initiated by Markowitz in the 1950s, picking up steam with all the groundbreaking work on CAPM by Sharpe, Lintner, Mossin, and Treynor, culminating in the development of Option Pricing Theory by Black, Scholes, and Merton. In contrast, the Modigliani-Miller propositions on capital structure and the Efficient Market Hypothesis lend themselves better to empirical investigation. The formal and mathematical approach spurred on theoretical research until the late 1980s. What happened after that was not that the theory was simply finished, although there is a good case to be made that the major theories developed in the 1960s and 1970s still form the backbone of finance (see Bernstein, 2007). But besides that, it was the proliferation of information technology and computers which made both gathering and analyzing data so much easier, thus stimulating empirical work and methods like bootstrapping and (Monte Carlo-)simulation.

Today's empirical work is vastly different from that of the late 1950s, 1960s and even early 1970s. Whereas the current strand consists for the overwhelming majority part of statistical data analysis, going back in time from 1980 we find more and more varying and non-quantitative forms. Empirical research in those days used a host of different methods to provide arguments: anecdotal and historical evidence, case studies and illustrative examples, surveys, questionnaires and interviews, and plain verbal logic. These methods are of course familiar to other social sciences. Today's empirical work is predominantly statistical with some occasional experimental research.

As far as theoretical work goes, a distinction can also be drawn, but it is less pronounced. Up until the middle 1970s non-formalistic theoretical papers show up occasionally. There is also made use of geometry in the form of graphs in the Marshallian fashion, for instance in Jensen & Meckling (1976). After that period, but also overlapping, we can distinguish between a brand using axioms, lemmas, and theorem proof (e.g. Merton, 1973) and a form of model building using simulation which often is game theoretical by nature (e.g. Grossman & Stiglitz, 1977).

Figure 8 show the data for the JFE; figure 9 the data for the JoF for the corresponding period.

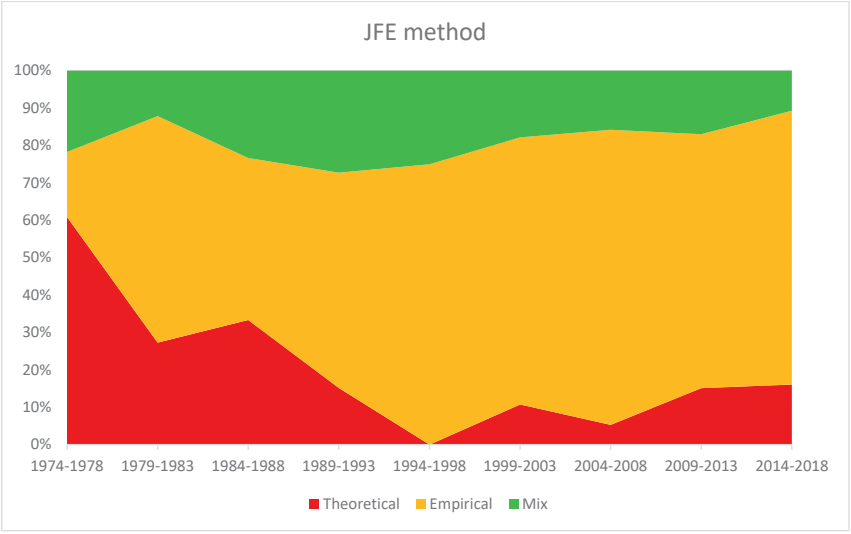


**Fig. 8.** Journal of Financial Economics approach 1974-2018

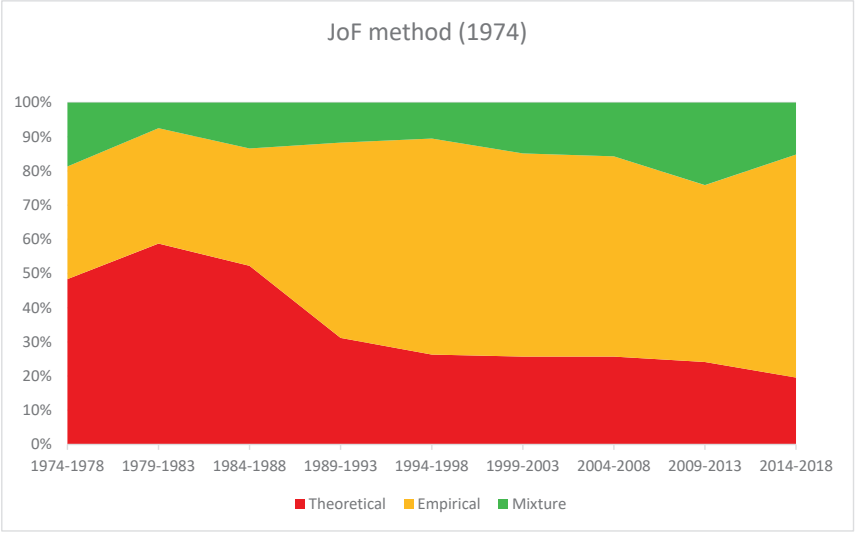


**Fig. 9.** Journal of Finance approach 1974-2018

As far as approach goes, the shift from theoretically inclined to empirically inclined work is even more pronounced in the JFE than in the JoF. The JFE in general seems to be more empirically oriented which shows not only in a higher percentage of purely empirical work but also in a larger number for mixed papers. This may be partly attributable to the JFE's focus on corporate finance and governance, areas which may lend itself particularly well to practical analysis.

Did the crisis have an impact on the approach contributors to these two journals employ? Like it was done for the subject categories the two following graphs display the type of contribution in the ten-year period before the crisis and the ten-year period after the crisis (three year intervals):

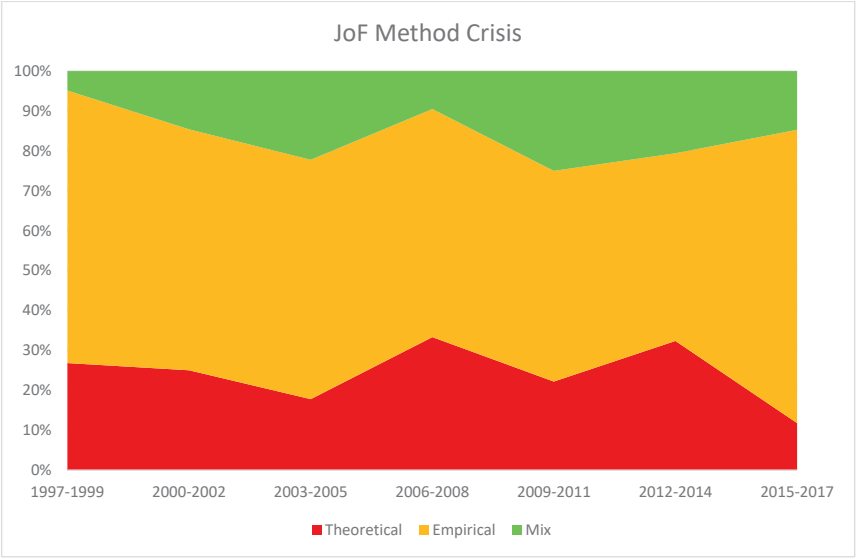


**Fig. 10.** Journal of Finance approach 1997-2017

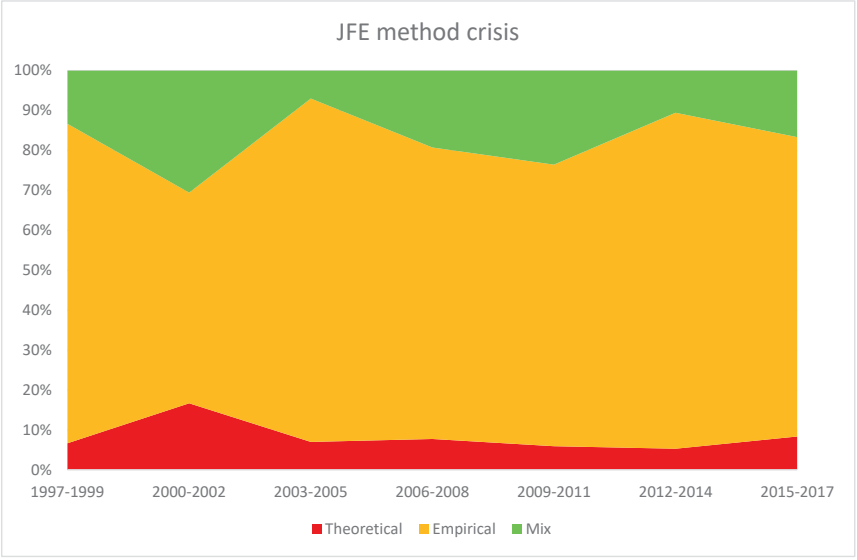


**Fig. 11.** Journal of Financial Economics approach 1997-2017

Besides a slight dip in the percentage of empirical work around the time of the crisis, there do not appear to be significant changes in the approach taken by both contributors to the JoF and the JFE. It was however noticeable in the editions used in the sample, in particular within the JFE sample, that since the crisis within the empirical part other methods than the traditional statistical data analysis show up more. Examples of what may be deemed historical (Calomiris & Carlson, 2016), sociological (Tahoun, 2014) and institutionalist (Eun, Wang, & Xiao, 2015) approaches were found.

The shift from theoretical to empirical work is not unique to finance. Kim, Morse, and Zingales (2006) try to map what has impacted economics in general between 1970 and 1999 by taking the most cited articles and then identify field, author, institution, and type of contribution. The last item is particularly interesting in this context. They report a steadily falling amount of theoretical contributions from over 75 % in the 1970-1974 interval to a little more than 10 percent in the period between 1995-1999. At the same time empirical work rises from some 10 % in the 1970s to 60 % by the end of the millennium. And more recently Backhouse & Cherrier (2017) conjectured a shift towards applied work in general economics at the expense of theoretical work form the 1970s onward. Others have also noted the so-called "empirical turn" in economics (see for instance Davis, 2019b). That would match the findings in this chapter.

Assessing the type of claim being made (prescriptive, descriptive, normative, explanatory, predictive, etc.) was problematic. That shouldn't come as a surprise since the aims of social scientists are plural and it is often not possible to draw a clear-cut line between those various aims[27]. A few observations can be made though. Most claims can at the least be characterized as explanatory. A typical empirical paper where a hypothesis is formulated, data are analyzed and the significance, statistical and economic, is determined, is saying something about the past. It shows ex-post how X was related to Y. In many cases the result is then used to postulate the relation as relevant for the future. So in such cases we can add "predictive" as a characteristic. The importance of predictions as outcome of academic research has been mentioned earlier as a hallmark characteristic of finance. But the predictions are usually non-quantitative[28]. Still, the typical modern empirical finance paper makes explanatory and qualitatively predictive claims.

Older empirical work from the 1950s and 1960s was quite different, as was explained above. Institutional finance, as it was called, made use of qualitative methods in addition to the

27 See Reiss (2009) for an extensive treatment.

28 There are a few exceptions. See for example Harvey & Whaley, 1992.

quantitative ones, which were much less sophisticated anyway[29]. The lack of "hard numbers" led to a different kind of argumentation and a different type of claims being made. Explanation is again present but here it is often accompanied by description instead of prescription. And in the case where the analysis was concerned with policy making this would often translate in a kind of normative claim as well.

Theoretical papers were also deemed mostly explanatory: a stylized representation of an assumed causal relation between certain variables. Sometimes predictions are derived, but these generally have a normative character since a theory is usually constrained by assumptions. So, a typical theoretical paper in finance can be deemed to make explanatory and normative claims.

## 2.6 VALIDITY AND ROBUSTNESS

There are at least two question marks to be put behind an exercise such as performed above. First, an analysis like this has some degree of subjectivity. The author has made the call on how to categorize and how to characterize a total of 1186 papers with wide-ranging subjects, spanning more than seventy years. Second, like in any sample the question has to be asked if it is a representative one.

As I see it, an advantage of the approach taken in this chapter is that it is not solely interpretation[30]. The assignment of papers in the sample to categories according to subject matches well for the period between 1978 and 1999 when the JoF published the distribution of received and published papers according to JEL categories in its annual report of the editor. These numbers and the numbers of the sample compare quite well to each other. However, there will remain some overlap and grey areas between categories. This is also acknowledged by studies on impact and citations (see Arnold et al., 2003, Kim, Morse, and Zingales, 2006, Keloharju, 2008).

Regarding the assessment of papers as theoretical or empirical, two questions were asked: one about the approach taken towards the central research question, and one about the nature of the arguments provided. Of course, deep down, all research, at least in the social sci-

29 As indicated earlier in this chapter, there are signs that a kind of institutional finance, or heterodoxy in general, is on the rise again.

30 In the recent literature on the use of quantitative analysis in methodology and history of economics, most contributors advocate quantitative analysis next to and in addition to qualitative approaches (Düppe & Weintraub, 2018, Edwards Giraud & Schinckus, 2018, Cherrier & Svorenčík, 2018, Jovanovic, 2018). Jovanovic's work is of particular interest in this context because it uses the case of the history of the Efficient Market Hypothesis.

ences, starts with the goal of saying something about a phenomenon, and is thus empirically grounded. But there is a big difference between data analysis and axiom building. Sometimes the demarcation is less clear cut, reason why some articles are classified as mixed. In other cases either the theoretical or the empirical part clearly dominates[31]. Fortunately, the authors themselves indicate frequently if theirs is a theoretical or an empirical paper. Kim, Morse, and Zingales (2006) use a similar approach, basing their judgment on "the authors' collective familiarity with the articles or a quick reading".

Two issues need to be addressed here, regarding representativeness. First, is the sample representative of the population, i.e. all JoF and JFE articles? Second, are the JoF and JFE representative for the bulk of finance research? The sample consists of about twenty percent of all JoF articles since 1946, and slightly less than fifteen percent of all JFE articles since 1974. Moreover, the problem of clustered articles on the same subject in one issue was addressed by summing up over longer intervals of five years which accidently is the same interval length that Kim, Morse and Zingales (2006) use. Finally, the correspondence of the distribution of subjects between the sample and the JoF's own data between 1978 and 1999 suggests that at least for that time period the sample provides an accurate picture.

Regarding the second issue – representativeness of the JoF and JFE –, a caveat is in order. Financial economists have also published in general economic journals, producing research of broader economic interest. Examples are Black and Scholes seminal 1973 paper which was published in the Journal of Political Economy and Modigliani-Miller 1958, published in the American Economic Review. The results of the studies of Arnold et al. (2003, and Keloharju (2008) are useful in this regard. Arnold et al. (2003) find that, while a significant amount of the most highly cited papers in finance has come from non-finance journals like the American Economic Review, Econometrica, and the Journal of Political Economy, "the JoF and JFE have unambiguously had the most impact on finance research". Keloharju (2008) reaches a similar conclusion.

## 2.7 DISCUSSION

Back in paragraph 2.2 three characteristics of finance, in relation to economics in general, were formulated: 1) focus on micro-level analysis in finance, 2) "positivism" and 3) development through time fueled by breakthrough theorizing. How do the empirical results measure up? The first observation was concerned with the level of analysis. It would indeed seem that most attention has been increasingly directed toward market and firm behavior,

31 Good examples here are the two seminal papers on option pricing. Black & Scholes (1973) and Merton (1973) are both highly theoretical papers but there is attention for applications of the theory.

looking at the dominance of the categories Global Financial Markets, and the subcategories market analysis and asset pricing in particular, and Corporate Finance & Governance. But it is also clear that this hasn't always been the case. The focus of analysis in finance, at least in the journals analyzed here, has gradually moved through time towards an increasing micro-perspective. Originally macro issues, such as public policy and welfare implications, were high on the research agenda. Moreover, such issues were typically of a more economic nature: monetary policy, public finance, fiscal policy. When the focus became narrower, the subjects were to a large extent lost or migrated to other branches of economics.

So Summers' complaint that finance is not concerned (anymore) with the bigger picture and issues seems to have substance. Ross, Campbell, and Miller have made a similar assessment but for them it is simply a natural consequence of the progress made in finance. An important part, maybe the most important part, of that progress has been the development of the hallmark finance theories: Modern Portfolio Theory, the Modigliani-Miller propositions, CAPM, Efficient Market Hypothesis, Agency theory, Option Pricing Theory. Indeed, the impact of these major theoretical breakthroughs is clearly visible in the analysis.

Of course something has changed since the time of Summers' remarks, but it has not been a move back towards the grander issues of the olden days. The advent of behavioral economics has brought a strand of research in finance where the focus of analysis is even narrower. Instead of aggregates such as markets and firms in behavioral finance the emphasis has turned towards the behavior of individual agents. In the graph this can be tracked by the rising share of papers on market analysis and on investor behavior. Since the debate between neoclassical and behavioral finance is far from settled, there is no telling what the future will look like.

The narrowing of focus is also visible in the rise of the amount of applied, empirical work, as mentioned by Bernstein (2007). He suggests that the core body of theory, dating back to the 1960s and 1970s, still forms a fundament in finance. Today's research, both in academia and on Wall Street, therefore is aimed at looking at the fringes of established theory in the quest to find something new and interesting. That includes possible behavioral biases but also institutional issues such as inefficiencies arising from transaction costs and liquidity constraints.

That leads to a second observation. The analysis of hard price data indeed has a very important place in modern finance. Summers' suggestion in that direction was only the beginning; the move has proliferated since 1985, as can be seen in the graphs. This has led to a bottom-up approach in finance as suggested by Ross and Campbell: first check the data, then work out the theory. The word "theory" may have become an inappropriate characterization in this context. A typical empirical paper in modern finance, neoclassical

and behavioral, states a hypothesis which is then checked by the data[32]. This approach can lead to an ad-hoc character of the hypotheses. Indeed, Fama (1998) condemns much of the criticism on established finance theory for this reason. It takes a theory to beat a theory, he says, not a loose collection of observations and anomalies.

The abundance of available data has been frequently mentioned as an advantage that finance researchers enjoy over other economists. Indeed, if a comparison is made between the results here and those of Kim, Morse, and Zingales (2006) similar patterns show up in the type of contribution. The numbers differ however: finance has always been more empirically oriented than economics in general (with perhaps some exceptions such as economic history and labor economics)[33]. But there is a drawback to all that empirical work: the danger of data-snooping. In other words, if one looks carefully he or she may find data on a specific market in a specific market to match any hypothesis about prices and/or market behavior (see Lo & MacKinlay, 1998). Especially when the level of detail in the analysis increases more and more, this may lead to trivial and meaningless results.

So finance can indeed be regarded as a positivist discipline in that it occupies itself with testing, rigorous or not. What about that other trait of Friedman inspired positivism: meaningful predictions? It was mentioned above that these predictions were overwhelmingly qualitative in the sense that no precise numbers are predicted but rather a tendency or a cause-effect relation. But shouldn't one expect from research on financial markets that it leads to a form of forecasts which can then be used in the market itself and found valid or not[34]? This is what McCloskey (1990), tongue-in-cheek, calls the "ultimate persuasive test" for a theory. Predictions in such sense are very rare, at least in the sample. It must be said that much of this kind of research does address the question whether the results are not only statistically but also economically significant, i.e. the monetary effect is checked. But hardly ever is an author tempted to postulate that money can be made on the basis of his hypothesis or model[35]. That also applies to those who do not take market efficiency for granted, such as many behavioral economists. So what is actually done is back-testing: testing a predictive model on historical data. Back-testing is common practice in the real world of money and investment management, but there successful back-testing usually leads to implementation. In the academic arena there is apparently less place or interest for this ultimate test of reality: real-life application of predictive models.

32 Which is slightly different from Miller's assertion of a typical JoF paper; see paragraph 2.2.

33 Keloharju (2008) finds that the most-cited papers in finance show a larger percentage of empirical contributions, compared to the numbers of Kim, Morse, and Zingales.

34 De Scheemaekere (2009) treats this issue in a somewhat philosophical manner.

35 Many finance scholars do have links to, or are themselves active in professional money management. Their performance in the practice of financial markets is the subject of the following chapter.

The second observation, "positivism" then seems to have been confirmed, with qualifications, by the analysis. But also in this case things have been different in the past. After WW II, finance had a distinct institutional flavor, the analysis being broader, more qualitative and descriptive, using a whole range of methods of the social sciences. Then in the 1960s the formalization and quantification of finance took place bringing about the present orthodoxy.[36] Formal theory and quantitative empirical testing of that theory took center stage. This fundamental change is also documented in a paper in the JoF by Robert Kavesh: "(D)isciplines change, new thinking evolves, controversies flair. In finance one can almost sense the shifts by leafing through the volumes. The early years were filled with descriptive articles, with a heavy "institutional" flavor—largely reflecting the type of research being carried out in those days" (Kavesh, 1970). Kavesh's article includes an exchange from 1967 between old-school financial economist Harry Sauvain and a new-school financial economist J. Fred Weston. Both emphasize that there ought to be space in finance for both the mathematical model-building approach and the institutionalist perspective. It turned out otherwise, although, as mentioned earlier in this chapter, recently there have been some signs of renewed room for institutionalism and heterodoxy.

In the 1980s finance really developed into the modern version, facilitated by the growth of information technology and computing power, enabling easier and more extensive gathering, treatment and analysis of data. Theoretical work took a backstage position again compared to empirical work. Harrison (1997) has noted this as well: "The pace of innovation in finance, for instance, seems to have slowed and the new sparks now come more from the tinder of inductive observation than from deductive "core" theorizing". Again, this movement is not unlike what might have happened in economics at large. Davis (2019a) raises the question if, as a result of increased specialization and fragmentation within economics, the field has become increasingly data-driven and more agnostic about underlying theory. While that might be regarded as a undesirable "less-scientific" development, it might also have led to creating a bit more space for heterodoxy, for instance behavioral, experimental, institutional, and evolutionary elements. So indeed a lot has changed in finance over time as was suggested.

## 2.8 CONCLUDING REMARKS

The major findings were a shift in the subjects of the writings over time and two major changes in approach. The former can be categorized best as a narrowing of the scope of analysis. From a broad, descriptive, qualitative perspective, the analysis has become ever

36 This was also noted by Davis, 2012.

more focused, first towards market and firm behavior and now increasingly towards behavior of agents and single phenomena. The first move can for a large part be attributed to the development of groundbreaking theory in the 1960s and 1970s, which enabled a more precise and detailed analysis. The second move was due to the firm embedding of that theory and the increasing availability of ever more fine grained data and ways of working with those data. There have been no such spectacular advances in theory since the 1980s. Researchers were led to a more applied, empirical approach, working on the periphery of that established theory, looking with ever more detail to certain assumptions, institutions, anomalies, etc.

In terms of "Big-M Methodology" one could regard the first move as the establishment of a Lakatosian hardcore, followed up by increasing work in the protective belt (see also Harrison, 1997, for a similar line of thought). Lakatos' idea was that the identity of an individual research program could be understood in terms of core and periphery elements, where the former is stable and relatively unchanging and the latter is adjusted to accommodate explanations of phenomena in terms of that set of core elements. Inspired by the work from and on Lakatos, Davis (2008, 2019a) has attempted to apply this idea to specific fields and disciplines, economics in particular, and it appears to be applicable to finance as well.

The changes in approach cannot be seen separate from the developments described above. In fact it is quite conceivable that the changes in approach have enabled the breakthrough theoretical work in the first place. Two things stand out. First, there is formalization and quantification which takes place in the 1950s and 1960s leading to the development of that hardcore body of theory. It is the rise of the mathematical model-building approach in economics that made the development possible of modern portfolio theory in 1952 (see also Kavesh, 1970). The Modigliani-Miller propositions, while not so much the product of a quantitative approach, were the result of rigorous formal logic. On these two pillars the remainder of the body of main theories in finance was built. Second, there are the technological advances of the middle 1980s that propelled the rise of empirical data analysis. This gave rise to a frenzy of testing and inspection of ever more detailed cases; in other words, an increasingly micro-oriented perspective. To use the terminology of big-M Methodology again, it is these two changes in approach that can be labeled as the true scientific revolutions in finance.

How do these developments compare to economics in general? Is finance profoundly different from economics? Milton Friedman appeared to think so in the 1950s when he said he had problems with granting Harry Markowitz his PhD in economics, stating during the defense that his work was "not economics, not mathematics, not even business science!"[37] It

37 Transcript interview with Harry Markowitz. Jacob Marschak, Markowitz's thesis advisor subsequently added: "It's not literature". See also Pistorius (2015) for an elaborate account of Markowitz's work.

is evident that the fields are linked. Finance came relatively late to the wave of formalization in economics, but when it did, it used those tools well. Arguably, the various asset pricing models are the most practically relevant applications of equilibrium theorizing that can be found in economics. Nowhere is econometrics used more than in applied, empirical work in finance. On the other hand, finance may have led the way with its micro-focus[38]. Fueled by game theory and experimental work, today economics also engages more and more in the kind of detailed applied work that has characterized finance for a while. Finance, Harrison (1997) writes, "has become the "proving ground" for new price theory and econometric technique. Innovations in finance found their way back to the "rest" of economics, in particular statistical and computer techniques".

Finance may also provide some insights for a more pluralist type of economics (Davis, 2007, 2008). While the core of finance is arguably staunchly neoclassical, the field has incorporated somewhat heterodox strands of thoughts, such as agency theory and behavioral economics. In fact, these hardly can be labeled heterodox anymore in this context and have more or less become part of the hardcore. One of the striking advantages that finance perhaps enjoys over economics in general is the incredibly strong monetary incentives that are provided by the financial markets. Given that proximity of theory and practice and the empirical prowess of finance, it is quite conceivable that finance will function as a battle ground for new ideas again.

38 Take for instance the clearly related ideas of the Efficient Market Hypothesis and rational expectations: Fama and Samuelson predate Robert Lucas' seminal work.

## APPENDIX

Sample composition.

### Journal of Finance:

- Volume 72, no. 4, August 2017;
- Volume 71, no. 3, June 2016;
- Volume 70, no. 2, April 2015;
- Volume 69, no. 1, February 2014;
- Volume 68, no. 6, December 2013;
- Volume 67, no. 5, October 2012;
- Volume 66, no.4, August 2011;
- Volume 65, no. 3, June 2010;
- Volume 64, no. 2, April 2009;
- Volume 63, no. 1, February 2008;
- Volume 62, no. 6, December 2007;
- Volume 61, no. 5, October 2006;
- Volume 60, no. 4, August 2005 (AFA annual meeting);
- Volume 59, no. 3, June 2004;
- Volume 58, no. 2, April 2003;
- Volume 57, no. 1, February 2002;
- Volume 56, no. 6, December 2001;
- Volume 55, no. 5, October 2000;
- Volume 54, no. 4, August 1999 (AFA annual meeting);
- Volume 53, no. 3, June 1998;
- Volume 52, no. 2, April 1997;
- Volume 51, no. 1, February 1996;
- Volume 50, no. 5, December 1995;
- Volume 49, no. 4, September 1994;
- Volume 48, no. 3, July 1993 (AFA annual meeting);
- Volume 47, no. 2, June 1992;
- Volume 46, no. 1, March1991;

- Volume 45, no. 5, December 1990;
- Volume 44, no. 4, September 1989;
- Volume 43, no. 3, July 1988 (AFA annual meeting);
- Volume 42, no. 2, June 1987;
- Volume 41, no. 1, March 1986;
- Volume 40, no. 5, December 1985;
- Volume 39, no. 4, September 1984;
- Volume 38, no. 3, June 1983;
- Volume 37, no. 2, May 1982 (AFA annual meeting);
- Volume 36, no. 1, March 1981;
- Volume 35, no. 5, December 1980;
- Volume 34, no. 4, September 1979;
- Volume 33, no. 3, June 1978 (AFA annual meeting);
- Volume 32, no. 2, May 1977 (AFA annual meeting);
- Volume 31, no. 1, March 1976;
- Volume 30, no. 5, December 1975;
- Volume 29, no. 4, September 1974;
- Volume 28, no. 3, June 1973;
- Volume 27, no. 2, May 1972 (AFA annual meeting);
- Volume 26, no. 1, March 1971;
- Volume 25, no. 5, December 1970;
- Volume 24, no. 4, September 1969;
- Volume 23, no. 3, June 1968;
- Volume 22, no. 2, May 1967 (AFA annual meeting);
- Volume 21, no. 1, March 1966;
- Volume 20, no. 4, December 1965;
- Volume 19, no. 3, September 1964;
- Volume 18, no. 2, May 1963 (AFA annual meeting);
- Volume 17, no. 1, March 1962;
- Volume 16, no. 4, December 1961;
- Volume 15, no. 3, September 1960;

- Volume 14, no. 2, May 1959 (AFA annual meeting);
- Volume 13, no. 1, March 1958;
- Volume 12, no. 4, December 1957;
- Volume 11, no. 3, September 1956;
- Volume 10, no. 2, May 1955 (AFA annual meeting);
- Volume 9, no. 1, March 1954;
- Volume 8, no. 4, December 1953;
- Volume 7, no. 3, September 1952;
- Volume 6, no. 2, June 1951 (AFA annual meeting);
- Volume 5, no. 1, March 1950;
- Volume 4, no. 4, December 1949;
- Volume 3, no. 3, October 1948;
- Volume 2, no. 2, October 1947;
- Volume 1, no. 1, August 1946.

**Journal of Financial Economics:**

- Volume 128, no. 2, May 2018;
- Volume 124, no. 1, April 2017;
- Volume 119, no. 3, March 2016;
- Volume 115, no. 2, February 2015;
- Volume 111, no. 1, January 2014;
- Volume 110, no. 3, December 2013;
- Volume 106, no.2, November 2012;
- Volume 102, no. 1, October 2011;
- Volume 97, no.3, September 2010 (The 2007-8 financial crisis: Lessons from corporate finance);
- Volume 93, no. 2, August 2009;
- Volume 89, no. 1, July 2008;
- Volume 84, no. 3, June 2007;
- Volume 80, no. 2, May 2006;
- Volume 76, no. 1, April 2005;
- Volume 71, no. 3, March 2004;
- Volume 67, no. 2, February 2003;
- Volume 63, no. 1, January 2002;
- Volume 62, no. 3, December 2001;
- Volume 58, no. 1-2, October/November 2000;
- Volume 54, no. 1, October 1999;
- Volume 49, no. 3, September 1998;
- Volume 45, no. 2, August 1997;
- Volume 41, no. 3, July 1996;
- Volume 39, no. 2, June 1995;
- Volume 35, no. 2, April 1994;
- Volume 33, no. 1, February 1993;
- Volume 31, no. 1, 1992;
- Volume 30, no. 2, December 1991;
- Volume 28, no. 1-2, November/December 1990;
- Volume 24, no. 2, 1989;

- Volume 21, no. 2, September1988;
- Volume 18, no. 2, June 1987;
- Volume 16, no. 1, May 1986;
- Volume 14, no. 1, March 1985;
- Volume 13, no. 1, March 1984;
- Volume 11, no. 1-4, April 1983;
- Volume 10, no. 4, December 1982;
- Volume 9, no. 3, September 1981;
- Volume 8, no. 2, June 1980;
- Volume 7, no. 1, March 1979;
- Volume 6, no. 4, December 1978;
- Volume 5, no. 2, November 1977;
- Volume 3, no. 4, October 1976;
- Volume 2, no. 3, September 1975;
- Volume 1, no. 2, July 1974.

# Chapter 3

## Putting your Money where your Mouth is: Neoclassical and Behavioral Investment Management

*This chapter explores one of the main divides in thinking about financial markets: do financial markets behave in a more or less efficient manner which would imply that excess returns are unachievable in the long run, or are there systematic and enduring deviations which can be exploited to provide superior returns? Some of the leading protagonists on both sides of this debate happen to be involved in professional asset management operations. The real-world performance of these operations is compared in terms of risk and return in order to provide a new perspective on the neoclassical vs. behavioral debate in finance.*

## 3.1 INTRODUCTION

Two streams of thought dominate the thinking on how financial markets function. On the one hand there are those who adhere to some form of the Efficient Market Hypothesis: prices respond to available information and the resulting price is the best current estimate of the "right" price. The implication is that, in the long run, no excess returns can be made without taking on a corresponding risk; in other words, there are no persisting free lunches in financial markets. On the other hand there are those that believe that systematic and enduring deviations from market efficiency exist which can be exploited: the market can be beaten.

Both parties have extensive empirical support to back up their claims and counterarguments towards one another. Ironically, the abundance of data in financial markets and the ways to deal with those data (Lo & MacKinlay, 2001) are such that perfectly legitimate evidence can be found on both sides. Indeed, finance has always been proud of its empirical prowess: nowhere in economics are theories and models tested so thoroughly thanks to the prolific amount of data that the financial markets provide.

This chapter presents a different kind of empirical analysis. Deirdre McCloskey (1989) once posed what she labelled as "The American Question" to economists: "If you're so smart, why ain't you rich?" According to McCloskey, the one ultimate persuasive test for an economic theory claiming to predict the future is whether there is money to be made with it[39]. For financial economics the obvious place to take that test is the financial market which has provided the input on which the various ideas about price information in such markets is based.

What follows is a comparison of the actual performance of a set of mutual funds from asset management firms with strong connections to leading academic figures in neoclassical and behavioral finance. The analysis presented here does not pretend to come to a final verdict on who's right and who's wrong with regard to market efficiency and asset pricing models. Actually, I believe both can be reconciled, which will be argued in the next chapter. Necessarily the sample only exists of four funds; meaningful comparisons with more funds was not achievable because of a lack of other comparable funds. Also, varying microfoundations and underlying ontological commitments of behavioral and neoclassical finance are not explored. Rather, it is an investigation of what difference, if any, in actual practice is observable, resulting from the two different approaches. In addition it might be possible to infer if there is any added value coming from strong academic pedigree.

39 "Making money" of course goes beyond financial markets: it applies to any economic idea which in some way produces profits.

There have been related studies. The investment professional Larry Swedroe published a short article in 2015 comparing the results of the same funds, solely focusing on returns. The analysis here is more elaborate and also focuses on risk, besides return. In 2008 Wright, Banerjee and Boney published an article called "Behavioral Finance: Are the disciples profiting from the doctrine?", where they examine fourteen self-proclaimed behavioral funds. Their focus was not solely on performance but also on how much capital behaviorally inspired funds attract, compared to "regular" funds. In that way they hoped to be able to infer if behaviorally inspired investing really adds value or if the "behavioral" label helps to attract investment capital, i.e. that it has marketing value. The analysis here can be considered complementary to the study of Wright et al.

## 3.2 RESEARCH DESIGN

Involvement of economists in financial markets can be traced back to classical economists like Richard Cantillon and David Ricardo, running through Maynard Keynes and Irving Fisher (Raines & Leathers, 2000) to Fischer Black, Michael Jensen, Myron Scholes and Robert C. Merton Ricardo started working as a stockbroker and did well in the bond markets while Cantillon was a banker and managed to make money on one of the most notorious investment disasters in history: the South Sea Bubble. Maynard Keynes, never one with a firm belief in the importance of financial markets and the efficient working of those markets, still was an active investor whose fortunes varied. Fisher, finally, was financially ruined by the Great Depression of the thirties (Blaug, 1997). Both Keynes and Fischer managed large funds for their universities.

Nowadays that is still the case: some of the main protagonists on both sides of the neoclassical-behavioral debate happen to be involved in professional investment management operations. 2013 Nobel laureate Eugene Fama, one of the fathers of the Efficient Market Hypothesis (Fama, 1965, 1970,1998) and Kenneth French, who together with Fama has provided various ground-breaking contributions to asset pricing (Fama & French, 1992, 1993), sit on the board of Dimensional Fund Advisers (DFA) which as of June 30, 2018 manages $582 billion in assets (September 30, 2010: $187.9 billion in assets under management (AuM)). Affiliated at some point in one way or another have been Robert Merton, Myron Scholes, Merton Miller, George Constantinides, Roger Ibbotson and John Gould. All of these are or were amongst the most distinguished scholars in the field of finance (including three more Nobel laureates in Miller, Merton and Scholes), coming from institutions like the University of Chicago, Harvard, Stanford and Yale. DFA's links to the University of Chicago, in particular its business school, are crystal clear. After an unprecedented donation of $300

million by DFA co-founder and long-time executive David G. Booth, the business school was named after him: University of Chicago Booth School of Business.

DFA's investment philosophy can clearly be labelled neoclassical:

> At Dimensional, our investment approach is based on a belief in markets. Rather than relying on futile forecasting or trying to outguess others, we draw information about expected returns from the market itself—letting the collective knowledge of its millions of buyers and sellers set security prices.
>
> Letting markets do what they do best—drive information into prices—frees us to spend time where we believe we have an advantage, namely in how we interpret the research, how we design and manage portfolios, and how we service our clients. It means we take a less subjective, more systematic approach to investing—an approach we can implement consistently and investors can understand and stick with, even in challenging market environments.

The means by which this philosophy is implemented are multiple-factor models, based on a version of the multiple-factor models developed by Fama and French (e.g. 1992, 1996). The models can be regarded as a variation of William Sharpe's single factor Capital Asset Pricing Model (CAPM) (Sharpe 1964, but also Lintner, Treynor, Black)[40]. Factors in addition to the CAPM beta, can be historic excess returns of smaller cap stocks over larger caps stocks and the historic excess returns of value stocks over growth stocks. Given its investment philosophy and investment process, it is no stretch to label DFA a neoclassical investment management firm.

Looking at the behavioral side of the spectrum provides interesting asset/investment management operations as well. 2017 Nobel laureate Richard Thaler, also from University of Chicago (though definitely not a Chicago School economist) and perhaps currently the best-known behavioral economist, is on the board of the firm he co-founded in 1993 and which bears his name: Fuller & Thaler Asset Management. Not only has Thaler been on the forefront of the behavioral challenge to the dominant paradigm of efficient markets academically (e.g. De Bondt & Thaler, 1984[41], Barberis & Thaler, 2003) but he has also played a leading role in popularizing behavioral economics outside academic circles with his book "Nudge" (Thaler & Sunstein, 2008). Daniel Kahnemann from Princeton, 2002 Nobel laureate and the pioneering figure of behavioral economics together with Amos Tversky

40 Although one could argue that multifactor models are "less neoclassical" than CAPM because by adding factors apparently not all information was priced in in CAPM.

41 See also his "anomalies" series in the Journal of Economic Perspectives.

(Kahneman & Tversky, 1979), is on the board of Fuller & Thaler as well, while prominent behavioral economics and finance scholar Nicholas Barberis from Yale has been a member of the academic advisory panel. The firm has as of June 30 2018 in excess of $9 billion in asset under management, having grown impressively (September 30, 2010: approximately $1 billion in AuM). Its philosophy becomes quite clear on the webpage. On the front page it says:

> Investors Make Mistakes. We Look For Them. At the individual stock level, we search for events that suggest investor misbehavior.
>
> Founded in 1993, Fuller & Thaler Asset Management has pioneered the application of behavioral finance in investment management.
>
> Primarily focused on U.S. small-cap equities, our strategies have historically delivered exceptional results. More importantly, our process differs significantly from traditional equity managers.
>
> We believe our unique perspective and unconventional approach can provide meaningful diversification to our mutual fund and separate account clients.

More specifically Fuller & Thaler appears to look for investors' behavioral biases that may cause the market to overreact to old, negative information about a company and underreact to new, positive information. The premise is that markets frequently overreact and underreact (cf. Barberis, Shleifer and Vishny, 1998). Underreaction occurs when a positive event for an asset, such an earnings surprise, is not immediately and fully priced in but rather gradually within a couple of months. In other words, positive news signals outperformance in the short term after the announcement. Overreaction is displayed in that companies that have made sequential negative announcements in the past tend to be undervalued while companies which have a good track record can be overvalued.

There is also LSV Asset Management where LSV is an acronym for Josef Lakonishok (University of Illinois at Urbana-Champaign), Andrei Shleifer (Harvard University) and Robert Vishny (University of Chicago). All three of them have been leading scholars, in particular with regard to behavioral finance (Lakonishok, Shleifer & Vishny, 1992, Shleifer & Vishny, 1997). LSV was founded in 1994 and currently manages approximately $118 billion in assets under management (September 30, 2010: approximately $59 billion in AuM). Lakonishok is still active in managing the funds while Shleifer and Vishny have retired from the firm. Here is what LSV states about itself:

> The fundamental premise on which our investment philosophy is based is that superior long-term results can be achieved by systematically exploiting the judgmental biases and behavioral weaknesses that influence the decisions of many investors. These include: the tendency to extrapolate the past too far into the future, to wrongly equate a good company with a good investment irrespective of price, to ignore statistical evidence and to develop a "mindset" about a company.
>
> LSV uses a quantitative investment model in what would be considered a bottom-up approach to choose out-of-favor (undervalued) stocks in the marketplace at the time of purchase and have potential for near-term appreciation. LSV believes that these out-of-favor securities will produce superior future returns if their future growth exceeds the market's low expectations.
>
> The competitive strength of this strategy is that it avoids introducing the process to any judgmental biases and behavioral weaknesses that often influence investment decisions.

LSV uses a proprietary asset pricing model but their investment process appears to have a lot in common with the strategy of Fuller & Thaler: looking for stocks that are undervalued because of a bad track record in the past and which haven't reacted properly to more recent, more relevant good news. Given the stated investment philosophies and the implementation methods, both Fuller & Thaler and LSV can be regarded as behavioral investment management operations.

For both the "behavioral" and "neoclassical" funds it is assumed that actual investment operations are in accordance with their stated investment philosophies[42].

## 3.3 DATA SETS

DFA, Fuller & Thaler and LSV all offer a range of varying products. For the purpose of this chapter only mutual funds have been considered for comparison.[43] That means that many of the products that are offered by these three firms are excluded. Mutual funds come in many guises: US only or international, small, mid and large caps, value versus growth orientation. For the purpose of this chapter the popular Morningstar methodology is the starting point. Morningstar classifies funds along two dimensions: size and nature of the investments made

42 While the cynical among us could be wary of this assumption, supervisors (and to some extent auditors) are supposed to check if business models match company statements.

43 A mutual fund is a professionally managed investment fund that pools money from many investors to purchase securities. These investors may be retail or institutional in nature.

by the fund. Size is differentiated in large, mid and small cap investments while for the nature of the investment a distinction between and a value and growth orientation is made with a blend of both in between. resulting in nine possible categories. Only funds that fall in the same Morningstar category and thus seem comparable, have been examined.

That leaves two sets of comparisons between behavior-style mutual funds and neoclassical-style mutual funds. DFA's US Small Cap Value fund and Fuller & Thaler's Undiscovered Managers Behavioral Value Fund both fall in the category small cap/value[44]. DFA's US Large Cap Value fund and LSV's Value Equity fund are both large cap/value types of funds. All these funds invest exclusively in American stocks. Table1 shows some core characteristics of the funds like ticker symbol, size, turnover, category and associated expenses such as management fees and transaction costs[45]:

**Table 1.** Fund characteristics

| Fund | Ticker | Total assets | Expenses | Turnover | Category |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | $ 16.2 billion | 0.52 % | 24.0 % | Small Value |
| F&T Undiscovered Managers Behavioral Value | UBVLX | $ 6.0 billion | 0.99 % | 24.0 % | Small Value |
| DFA US Large Cap Value | DFLVX | $ 31.2 billion | 0.27 % | 15.0 % | Large Value |
| LSV Value Equity | LSVEX | $ 2.71 billion | 0.66 % | 15.0 % | Large Value |

While all these funds have grown considerably over the years, looking at assets under management, the difference in the size of total assets between the DFA funds on the one hand and the F&T and LSV funds on the other hand is noticeable. This probably accounts at least partially for the differences in expenses between the funds: asset management is a business which displays economies of scale.

44 Technically the Undiscovered Managers Behavioral Value Fund of Fuller & Thaler was offered by JP Morgan. F&T function as the sole subadvisor of these funds and can thus be regarded as responsible for the investment decisions.

45 Data from Morningstar.

## 3.4 RATINGS & RETURNS

Comparison of performance should not be limited to looking solely at returns but should also include appraisal of the risk involved. Morningstar issues ratings to each mutual funds ranging from one to five stars, one being the worst and five starts being among the best. These rating are relative to the distribution of the performance of the whole population of funds within a category. The 10% of funds in each category with the highest risk-adjusted return receive five stars, the next 22.5% receive four stars, the middle 35% receive three stars, the next 22.5% receive two stars, and the bottom 10% receive one star. The star ratings take both return and risk into account. The basic concept is relatively straightforward: it assumes that investors have loss aversion and prefer steady returns above volatile ones, all else being equal. Ratings are computed for three, five and ten year periods. An overall rating is calculated using a (frontloaded) weighted average, taking into account the period of existence of the fund. Table 2 show the Morningstar ratings for the funds considered here[46]:

**Table 2.** Morningstar ratings

| Fund | Ticker | 3-year | 5-year | 10-year | Overall |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | *** | *** | *** | *** |
| F&T Undiscovered Managers Behavioral Value | UBVLX | **** | ***** | ***** | ***** |
| DFA US Large Cap Value | DFLVX | **** | **** | *** | **** |
| LSV Value Equity | LSVEX | *** | **** | *** | *** |

Most striking is the difference between the two small cap funds. The "behavioral" F&T small cap fund scores consistently excellent, while the "neoclassical" DFA small cap fund scores average ratings. Both large cap funds score average to slightly above average, with a slightly better rating for the "neoclassical" DFA fund over the "behavioral" LSV fund. With the exception of the "behavioral" F&T small cap fund, it would appear that funds that are heavily influenced by leading academics do not perform exceptionally well. Not bad, but also not great.

Having made those observations, there isn't yet very much to conclude with regard to how good or bad the neoclassical and behavioral approaches perform. In order to do so, the specific returns and various risk parameters have to be examined. One, three, five, ten and

46 Data from Morningstar.

fifteen year returns are plotted below in tables 3 and 4 for the small cap value funds and large cap value funds[47]. Returns from relevant benchmarks such as the Morningstar category, the Standard & Poor's 500 index (US large cap stocks) and the Russell 2000 index (US small cap stocks) are also included.

**Table 3.** Returns small cap value funds

| Fund | Ticker | 1-year | 3-year | 5-year | 10-year | 15-year |
|---|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | 0.48 % | 9.09 % | 6.68 % | 12.94 % | 9.20 % |
| F&T Undiscovered Managers Behavioral Value | UBVLX | 1.88 % | 10.02 % | 9.17 % | 16.25 % | 10.37 % |
| Benchmarks | | | | | | |
| Category Small Value | SV | -0.26 % | 8.84 % | 5.83 % | 11.99 % | 8.35 % |
| S&P 500 index | SPX | 10.13 % | 13.13 % | 11.95 % | 13.81 % | 8.97 % |
| Russell 2000 index | RUT | 2.66 % | 9.82 % | 6.70 % | 11.34 % | 7.51 % |

The returns clearly reflect the Morningstar ratings: outperformance by the "behavioral" F&T small cap fund, average performance by the "neoclassical" DFA small cap fund. Looking at returns it would appear we have a clear winner, which has also been able to consistently outperform the benchmarks.

For the large cap value funds the returns are given in table 4:

**Table 4.** Returns large cap value funds

| Fund | Ticker | 1-year | 3-year | 5-year | 10-year | 15-year |
|---|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | 5.28 % | 11.16 % | 9.72 % | 13.98 % | 9.40 % |
| LSV Value Equity | LSVEX | 0.24 % | 9.37 % | 9.11% | 12.75 % | 8.73% |
| Benchmarks | | | | | | |
| Category Large Value | LV | 5.03 % | 9.91 % | 8.52 % | 11.67 % | 7.79 % |
| S&P 500 index | SPX | 10.13 % | 13.13 % | 11.95 % | 13.81 % | 8.97 % |



47 Data from October 22, 2019.

While the differences aren't enormous, DFA outperforms LSV in every time period considered. However, the performance of LSV seems affected by a bad recent year (2018), which also translates in a somewhat subpar 3-year return. The average Morningstar ratings for both funds are confirmed by the returns.

Looking at returns for value funds in general (both small cap and large cap) compared to the performance of the S&P 500 index, one can conclude that value has been an inferior investment category in terms of returns.

### 3.4.1 Risk-return performance small cap value funds

As noted before, comparisons of performance have to involve risk appraisal in order to draw a fair and complete picture. There is a number of ways to do so. Classical mean-variance analysis uses the volatility of the returns. The lower the standard deviation the less volatile the results of the fund are and, presumably, the less risky the investment.

**Table 5.** Standard deviations small cap value funds

| Fund | Ticker | 3-year SD | 5-year SD | 10-year SD | 15-year SD |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | 14.50 % | 14.33 % | 21.67 % | 19.72 % |
| F&T Undiscovered Managers Behavioral Value | UBVLX | 11.56 % | 11.19 % | 20.61 % | 19.20 % |
| Benchmarks | | | | | |
| Category Small Value | SV | 13.76 % | 13.62 % | 19.73 % | 18.24 % |
| S&P 500 index | SPX | 9.18 % | 9.55 % | 14.40 % | 13.19 % |
| Russell 2000 index | RUT | 13.07 % | 13.44 % | 19.36 % | 17,89 % |

The "neoclassical" DFA fund displays slightly higher volatility than both the "behavioral" F&T fund and the benchmarks. The low 3- and 5-year volatility of the "behavioral" F&T fund is remarkable, especially given the good returns the fund has provided.

Combined with the mean return the Sharpe ratio can be calculated by subtracting the risk-free rate of return from the return of the portfolio and dividing that result by the standard deviation of the portfolio's excess return. This can be regarded as an indication for in how far taking on more or less risk, as measured by the standard deviation, results in better returns. The higher the Sharpe ratio the better the risk-return tradeoff.

**Table 6.** Sharpe ratios small cap value funds

| Fund | Ticker | 3-year Sharpe ratio | 5-year Sharpe ratio | 10-year Sharpe ratio | 15-year Sharpe ratio |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | 0.93 | 0.65 | 0.57 | 0.53 |
| F&T Undiscovered Managers Behavioral Value | UBVLX | 1.22 | 1.02 | 0.74 | 0.60 |
| Benchmarks | | | | | |
| Category Small Value | SV | 0.95 | 0.63 | 0.41 | 0.53 |
| S&P 500 index | SPX | 1.69 | 1.36 | 0.84 | 0.67 |
| Russell 2000 index | RUT | 1.10 | 0.71 | 0.56 | 0.48 |

Using Sharpe ratios the quality performance of the "behavioral" F&T small cap fund becomes apparent. In all time intervals the "behavioral" fund scores better than the "neoclassical" DFA small cap fund. In addition, F&T outperforms both the Morningstar and Russell 2000 benchmarks in every single time-interval. The performance of the neoclassical" DFA small cap fund, as measured by Sharpe ratios, can be characterized as average.

Besides standard deviation and Sharpe ratio there are other measures to assess risk and return such as alpha, beta and R-squared. These have in common that performance is measured relative to the market as a whole, using CAPM.R-squared, the coefficient of determination, is a measure of the correlation between the returns of an asset and a relevant benchmark representing the market as a whole, generally the S&P 500 index. A coefficient of determination of 100 means that all movements in the price of the asset can be explained by movements of the market as a whole. A coefficient of determination between 85 and 100 is considered high; below 70 is considered low. The coefficient of determination is measured with reference to the S&P 500 index, the most commonly used broad market proxy.

**Table 7.** Coefficients of determination small cap value funds

| Fund | Ticker | 3-year $R^2$ | 5-year $R^2$ | 10-year $R^2$ | 15-year $R^2$ |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | 42.79 | 49.53 | 79.86 | 76.94 |
| F&T Undiscovered Managers Behavioral Value | UBVLX | 57.53 | 62.30 | 80.42 | 78.69 |
| Benchmarks | | | | | |
| Category Small Value | SV | 44.89 | 49.17 | 76.92 | 74.09 |

DFA's coefficient of determination is very much in line with the Morningstar benchmark for small value assets. F&T displays clearly higher correlation than both DFA and the Morningstar category. F&T's coefficient of determination deviates from the benchmark, which could indicate a different investment approach compared to typical small cap value funds. The DFA fund looks more like a typical small cap value fund. R-squared is not an unambiguous yardstick in that a higher or lower number is not simply better or worse. It does say something about the uniqueness of a fund compared to the market as a whole or a particular market segment. An interesting side note is the dramatic drop in the overall coefficient of determination of small cap value funds in general, compared to the broad S&P 500 index.

Beta is another measure to assess risk and return. It is a measure of the volatility, or systematic risk, in comparison to the market as a whole. Beta is calculated using regression analysis. A beta of 1 indicates that the security price will move in line with the market; a beta of less than 1 means that the security will be less volatile than the market; a beta of greater than 1 indicates that the security's price will be more volatile than the market.

**Table 8.** Betas small cap value funds

| Fund | Ticker | 3-year beta | 5-year beta | 10-year beta | 15-year beta |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | 1.03 | 1.06 | 1.34 | 1.31 |
| F&T Undiscovered Managers Behavioral Value | UBVLX | 0.96 | 0.93 | 1.28 | 1.29 |
| Benchmarks | | | | | |
| Category Small Value | SV | 0.98 | 0.98 | 1.20 | 1.15 |

Both funds in general have higher betas than the Morningstar benchmark, while F&T's low volatility in the 3 and 5-year intervals, as measured by standard deviation, appears to translate in lower betas for those time intervals.

An interesting performance measure is alpha. Alpha measures a fund manager's effectiveness. It displays the difference between a fund's actual returns (after fees) and its expected performance (based on CAPM), given its level of risk as measured by beta. Alpha can be considered as representing the value that a portfolio manager adds to or subtracts from a fund's return (Bernstein, 2007) Continued and sustained high alpha can be seen as a an indication that a particular investment manager or team has special skills or some other form of edge.

For both funds alpha is negative in all but one of the observations. However, overall alpha for the Morningstar benchmark is negative, which implies that small cap value funds haven't provided the best opportunities to beat the market. The behavioral F&T fund still outperforms the benchmark. The neoclassical DFA fund more or less performs in line with the benchmark.

**Table 9.** Alphas small cap value funds

| Fund | Ticker | 3-year alpha | 5-year alpha | 10-year alpha | 15-year alpha |
|---|---|---|---|---|---|
| DFA US Small Cap Value | DFSVX | -2.59 | -4.38 | -3.89 | -1.03 |
| F&T Undiscovered Managers Behavioral Value | UBVLX | -0.72 | -0.59 | -0.18 | 0.12 |
| Benchmarks | | | | | |
| Category Small Value | SV | -2.37 | -4.38 | -3.21 | -0.80 |

At first sight it would appear that in the case of these two small cap value funds the behavioral fund is superior to the neoclassical one: similar risk, better returns. Moreover, Fuller & Thaler's Undiscovered Managers Behavioral Value fund also beats the benchmarks consistently in both return and risk. DFA US Small Cap Value fund performs very much along the lines of the benchmarks. Perhaps that shouldn't come as a surprise. If the investment philosophy is that the market eventually processes all available information and can't be beaten in a consistent way over time, there is no edge to look for except good execution. However, it does mean that in the case of the DFA fund there is no "academic alpha". In the case of the F&T fund it is not possible to simply attribute its excellent performance causally to academic insights, i.e. behavioral finance/economics. What is clear though is that the F&T fund does something differently than the broad market, which might have provided better results.

Having said that a couple of important caveats need to be made. That superior performance is more visible in the shorter time intervals. If one observes the numbers for the 15-year interval, the differences are much less pronounced[48]. Keeping in mind an old adage in asset management: "past returns are no guarantee for the future", we still have to conclude that in the case of these two small cap funds, the behavioral one wins out. Another caveat is the fact that access to the F&T fund has been restricted since 2016 in the sense that no new

48 The author has conducted an earlier review in 2010, using the exact same research design. The conclusion in 2010 was that there wasn't much to choose between the neoclassical and the behavioral fund.

investors can participate in the fund anymore. While the reasons for this so-called "soft closure" are not known to me, one could conjecture that the supply of profitable opportunities might somehow not be unlimited. Taking on additional investment capital would then create downward pressure on returns. Could it be then that, yes, there are anomalies that can be profitably exploited, but, no, they are not abundant or endless? Which brings me to the possible impact of fund size. There appear to be advantages and disadvantages related to the size of investment funds. On the one hand expenses should relatively go down as a fund (or investment firm) grows bigger. That is clearly visible in table 1: DFA's expense ratios being lower than those from F&T and LSV. On the other hand there is evidence that performance suffers from a growing number of the assets under management. Chen et al. (2004) find that for small cap funds smaller funds indeed perform better than larger ones. The analysis of the two funds analyzed here seems to confirm those findings somewhat. First , the smaller behavioral fund seems to perform slightly better than the neoclassical on. Second, the F&T fund has shown exceptional growth from $36,2 million AuM in 2010 to $6 billion AuM in 2020 (DFA fund: $7.4 billion AuM in 2010, $16,2 billion AuM in 2020). That growth might have given rise to the soft closure of the fund[49].

### 3.4.2 Risk-return performance large cap value funds

A similar analysis has been done for the large cap value funds:

**Table 10.** Standard deviations large cap value funds

| Fund | Ticker | 3-year SD | 5-year SD | 10-year SD | 15-year SD |
|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | 10.84 % | 10.95 % | 18.50 % | 16.68 % |
| LSV Value Equity | LSVEX | 10.75 % | 10.80 % | 16.93 % | 15.44 % |
| Benchmarks | | | | | |
| Category Large Value | LV | 9.82 % | 10.03 % | 15.24 % | 14.09 % |
| S&P 500 index | SPX | 9.18 % | 9.55 % | 14.40 % | 13.19 % |

Looking at standard deviation/volatility there isn't much to choose between the two funds. Both though display slightly higher volatility than the benchmarks.

On a 5-, 10- and 15-year basis both funds provide very similar Sharpe ratio's which are more or less in line with the benchmark values. Looking at the 3-year periods, DFA stays in line with the benchmarks but LSV lags somewhat.

49 The expense ratio for the F&T fund also dropped with the growth of the fund: from 1,6% in 2010 to 0,99% in 2020 (DFA: 0,54% in 2010, 0,52% in 2020).

In the case of correlation with the S&P 500 index the "neoclassical" DFA fund displays a higher coefficient of determination than the benchmark for large cap value funds, while the metrics of the "behavioral" LSV fund are more in line with the benchmark. As was the case of small cap value funds, for large cap value funds the coefficient of determination drops, though less dramatically.

**Table 11.** Sharpe ratios large cap value funds

| Fund | Ticker | 3-year Sharpe ratio | 5-year Sharpe ratio | 10-year Sharpe ratio | 15-year Sharpe ratio |
|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | 1.30 | 1.03 | 0.66 | 0.59 |
| LSV Value Equity | LSVEX | 1.15 | 1.01 | 0.67 | 0.59 |
| Benchmarks | | | | | |
| Category Large Value | LV | 1.26 | 0.98 | 0.67 | 0.56 |
| S&P 500 index | SPX | 1.69 | 1.36 | 0.84 | 0.67 |

**Table 12.** Coefficients of determination large cap value funds

| Fund | Ticker | 3-year $R^2$ | 5-year $R^2$ | 10-year $R^2$ | 15-years $R^2$ |
|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | 85.18 | 86.82 | 94.33 | 93.16 |
| LSV Value Equity | LSVEX | 77.66 | 83.44 | 93.96 | 92,74 |
| Benchmark | | | | | |
| Category Large Value | LV | 79.42 | 83.46 | 91.35 | 90.36 |

**Table 13.** Betas large cap value funds

| Fund | Ticker | 3-year beta | 5-year beta | 10-year beta | 15-year beta |
|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | 1.09 | 1.07 | 1.25 | 1.22 |
| LSV Value Equity | LSVEX | 1.03 | 1.03 | 1.14 | 1.13 |
| Benchmark | | | | | |
| Category Large Value | LV | 0.95 | 0.96 | 1.01 | 1.01 |

Both funds display higher betas than the benchmark. LSV has lower beta for all periods considered. The numbers are in line with the standard deviation of returns, which was higher

for DFA than for LSV. Taken together it appears that DFA takes on a bit more non-systemic, idiosyncratic risk than LSV and the benchmark, while still maintaining higher correlation with the broad S&P 500 index. In other words, the DFA fund is more "different" from the benchmark/ typical large cap value fund than the LSV fund. That can be regarded as a surprise. One might expect that the behavioral investor is more inclined to deviate from the market proxy than the neoclassical investor since there are presumed to be inefficiencies within the market. That appeared to be the case in the examination of the two small cap funds. Here the evidence suggests otherwise.

**Table 14.** Alphas large cap value funds

| Fund | Ticker | 3-year alpha | 5-year alpha | 10-year alpha | 15-year alpha |
|---|---|---|---|---|---|
| DFA US Large Cap Value | DFLVX | -2.82 | -2.66 | -2.90 | -0.98 |
| LSV Value Equity | LSVEX | -3.67 | -2.58 | -2.38 | -0.87 |
| Benchmark | | | | | |
| Category Large Value | LV | -2.52 | -2.77 | -2.07 | -1.11 |

Negative alpha is displayed by both funds as well as the benchmark. Both funds score close to the benchmark with one exception: LSV's 3-year alpha. This is probably attributable to the subpar 1-year performance of the fund in terms of returns, that was noticed earlier. In the long run these funds seem pretty evenly matched, with perhaps a slight advantage for the "neoclassical" fund. There is no indication of "academic" alpha. These are good mutual funds but by no means spectacular in their performance. As for size effects, the DFA fund being approximately 11,5 times bigger than the LSV fund ($31,2 billion AuM vs. $2,71 billion AuM), Chen et al. (2004) found that there is no clear effect of size on performance for large cap value funds. None is detectable here as well, except for the differences in expense ratios.

## 3.5 DISCUSSION OF RESEARCH METHODOLOGY

Before drawing conclusions some issues need to be addressed: is the methodology used sound and are the data representative? Concerning the applied methodology, this is probably not an approach that a financial economist trying to publish in a finance journal would employ. In that case one would expect a more sophisticated full-blown statistical analysis. However, the measures used here are customary ones for investment and asset management appraisal. Swedroe in his 2015 short article compared the results of funds of Fuller & Thaler and LSV

to the broader industry, focusing just on returns though instead of also incorporating risk. Wright, Banerjee and Boney (2008) examined sixteen mutual funds over the period 1992-2005 with either a self-proclaimed or media-identified association to behavioral finance, including the Fuller & Thaler and LSV funds discussed here. They first focus on returns and flow of funds in order to identify successful funds. Then they examine risk as well as the factors where successful performance stems from.

Asset management firms themselves typically market their products by pointing at past returns rather than risk-return measures. Some may argue that risk-return measures may be flawed because they are to a large extent developed from the traditional building blocks from neoclassical finance: Modern Portfolio Theory, Capital Asset Pricing Model. As such these measures will suffer from the same troubles that plague these theories such as the question if the distribution of returns resembles a lognormal pattern and the question how to define the market portfolio (and thus calculate correlation and beta). The former does not pose a problem because in the simple mean-variance analysis used here no assumption regarding the distribution of returns is necessary and thus the question of whether markets are efficient does not enter a priori in the analysis. The latter problem –what is the market portfolio?– is also neutral with regard to the analysis. Different conceptions of the contents of the market portfolio may alter correlation and beta but again it is impossible to establish a priori if a particular notion of the market portfolio skews the analysis one way or the other.

Questions could also be raised about the time intervals used. Do three, five, ten and fifteen year intervals provide a balanced enough picture[50]? Without taking into account investor preferences, it would appear that the longest time horizon, i.e. the 15-year interval, presents the fairest comparison, smoothing out shorter term fluctuations. The 15-year interval also fully incorporates the great financial crisis (2007-2009) and the recovery period from the crisis. Grosso modo the 15-year intervals produce the smallest performance differences between the funds analysed here, both for small cap value equity funds as for large cap value equity funds. On the other hand, the most important differences show up in the 3-year intervals, The behavioral small cap equity value fund outperforms the neoclassical one in the short term, while in that same timeframe the neoclassical fund does better than its behavioral counterpart in the category large cap value equity funds. Whether these short-term differences in performance are persistent and eventually translate in more significant differences on the longer term, remains to be seen.

With regard to the second issue: is it fair to pass judgment on these asset management operations (and the people affiliated to those operations) based on the data analysed here? After

50 Wright et al. (2008) have examined the 1992-2005 period. Since their findings do not dramatically differ from the ones presented here, one could consider this as a kind of confirmation.

all, the data only consists of two sets of funds, while the firms considered here have many more products on offer. There are, however, good reasons to restrict the analysis to the funds considered. Most importantly, it wasn't easy to find comparable funds. While DFA offers an broad range of funds in all asset classes, F&T focuses on small and medium caps and doesn't provide large cap offerings. LSV concentrates on value investing.

The types of offerings by the behavioral asset managers, combined with their performance, beg the question if some asset classes are more suited to behavioral investing than other, i.e. value stocks and in particular small cap value stocks, given the good performance by the F&T Undiscovered Managers Behavioral Value Fund. Put another way: could it be that the market for small cap value stocks is less efficient than the market for large cap value stocks? The preference for value stocks above growth stocks is explainable. Value stocks are in general considered less risky, because future cash flow, dividends, etc. are deemed more predictable/less uncertain Over- and underreaction, the basis for behavioral investment decisions, should thus be easier detectable/objective than in the case of growth stocks. Wright et al. (2008) find that two of the four behavioral funds they examined and that outperformed their defined benchmark (i.e. the Vanguard 500 Fund) focus on value investing. These two funds happen to be the exact same F&T and LSV funds examined here. However they add that, while UBVLX and LSVEX outperform the broad benchmark in the period 1992-2005, this is not the case in comparison to other value-oriented "non-behavioral" mutual funds[51]. In other words, the outperformance might very well stem from well-executed value investing in general, rather than behavioral-inspired investing.

One could hypothesize that information on large caps is more abundant, less ambiguous, less noisy, than is the case for small caps. If that is the case, the market for small cap value stocks is less informationally efficient than the market for large cap value stocks and thus may offer more opportunities for excess performance.

Fuller & Thaler originally ran another small cap fund: the Undiscovered Managers Behavioral Growth fund. This particular fund, focused on growth investments rather than value investments, closed in 2012. As Swedroe (2015) puts it: "It's rare, if not unheard of, for a fund to close when it has produced good returns." According to Swedroe the performance of the Undiscovered Managers Behavioral Growth fund was indeed lagging, at least up to 2010.

51 This result matches the findings in my earlier 2010 review which would compare better as far as the investigated time period is concerned.

## 3.6 DISCUSSION OF IMPLICATIONS

A final point for discussion is what exactly can be inferred from the analysis with regard to the ideas of those affiliated with the funds. In other words, to what extent do the results credit or discredit their theories about the functioning of financial markets? Some historical perspective may be helpful here. For involvement of academics in the practice of financial markets is and was nothing new. The list of great economists of the past with more than a passing interest in active investing is extensive and enduring.

For these economists of the past investing was an activity on the side. Professional involvement of academics in investing and financial markets goes one step further. There is a variety of such cases, ranging from cooperation between banks and academics to apply investment theory such as between Wells Fargo and Fischer Black, Michael Jensen and Myron Scholes, to full-fledged professionalism like Black becoming a partner at Goldman Sachs (Mehrling, 2005). Notorious are the exploits of Nobel laureates Robert C. Merton and Myron Scholes who were involved as partners in hedge fund Long Term Capital Management (LTCM). After posting stunning results in the first three years of its existence, the demise of the fund was equally spectacular. The blow-up of LTCM in 1998 shook the global financial markets to its core, mandating massive action from the monetary and supervisory authorities. Popular belief has it that hubris and an unbridled belief in quantitative modelling and markets were behind the downfall of LTCM, not unlike what many believe happened in the recent financial crisis. Indeed Merton and Scholes thought that the relentless arbitrage, which LTCM engaged in, would drive markets to more efficiency. But that massive arbitrage process made LTCM into such an important market participant that one of the basic conditions for an efficient market—atomistic agents in the market—was violated, resulting in faltering liquidity conditions. So the demise of LTCM was not so much a result of markets being inefficient. Rather, it was the failure to recognize certain risks, in particular concentration risk, associated with one of the conditions for the efficient functioning of market not being in place that was missed by LTCM (Scholes, 2000, Stulz, 2000). So the historic evidence is mixed, to say the least, with regard to the question whether great economists are also great investors.

Likewise in the cases described in this chapter. One behavioral fund (F&T) distinguishes itself from its neoclassical rival, the other (LSV) does not. The failure of a particular fund to distinguish itself decisively from its rival does not need to discredit or falsify the philosophy behind that fund. That notion is especially relevant with regard to the behavioral mutual funds. For it's the behavioral philosophy which claims that markets systematically deviate from efficiency because of psychological biases and that these deviations can be profitably exploited. This was also remarked by Wright et al. (2008): "If funds visibly associated with behavioral finance cannot generate abnormal returns, their strategies are, in our opinion,

of little interest"[52]. They do find that the flow of funds into behavioral funds is higher, suggesting that behavioral mutual funds are effectively attracting capital. They also find that the behavioral funds generally beat the S&P 500 Index funds on a raw, net-return basis. However, their risk-adjusted returns are neither significantly better nor worse than their matched counterparts. Finally they find that most of success of behavioral funds, relative to the S&P 500, is explained by loading on the value factor, which actually originates in Fama and French's 1993 model. Thus they conclude that while behavioral investment strategies may differ from non-behavioral funds the differences aren't providing their investors any abnormal returns. Going further, however, they suggest that publicizing and presumably using "behavioral finance" in a fund's investment strategy does seem to offer advantages in terms of attracting capital and marketing purposes. Given the results presented however, something similar could be said with regard to the analysed Dimensional funds: that strong academic pedigree advertises well, but isn't accompanied by superior results.

Lack of clearly superior results from behavioral mutual funds may shed doubts on market wide deviations from efficiency but can be consistent with the finding of psychological biases at the level of the individual agent. Indeed, the idea that despite people constantly behaving "irrationally", market outcomes can be pretty efficient has been forcefully argued by many, in finance for instance by Merton Miller (1987) and Stephen Ross (2005) and in economics at large by Vernon Smith (1989)[53] and Deirdre McCloskey. The billion dollar question then isn't any longer whether markets are efficient or not, but how exactly non-rational, non-optimizing behavior can lead to aggregated efficient outcomes. Various proposals are out there that attempt to bridge the gap between the individual level and the aggregated market level: Andrew Lo's Adaptive Market Hypothesis (Lo, 2004 & 2005), Peter Bossaerts' Efficient Learning Hypothesis (Bossaerts, 2002), Merton and Bodie's Functional and Structural Finance (Merton & Bodie, 2005), and the various literature on noise trading emerging from Black (1986) and Kyle (1985) [54].

In the specific behavioral asset pricing literature (See Shefrin, 2008) the aggregation question is acknowledged. Shefrin tries to incorporate "sentiment", i.e. false beliefs held by investors,

52 The title of Swedroe's 2015 article "Behavioral finance falls short" doesn't leave much doubt about his conclusion and opinion.

53 Smith actually appeared to be of the opinion that some assumptions (an infinite number of agents and various assets) which are deemed necessary for efficiency and equilibrium in the neoclassical sense, are not indispensable for efficient outcomes if there is sufficient communication and information in a market.

54 In the next chapter I propose an account of the arbitrage process that drives markets towards efficiency along the lines of Austrian market process theory which accommodates both behavioral and neoclassical perspectives.

in the price kernel-based asset pricing framework described by Cochrane (2005)[55]. In the absence of sentiment the market is efficient. Whether sentiment enters the equation or the outcome is efficient then becomes an empirical matter.

The bottom line is that the neoclassical and behavioral perspectives are not rival in the sense that one being true necessarily implies that the other is completely false. Indeed these schools of thought can be considered as complementary because their main bite is situated on different levels of aggregation. The behavioral viewpoint is based on a strong body of evidence, both empirical and experimental, to back up claims about economic agents not behaving along the lines of homo economicus. The neoclassical corner has solid arguments, both theoretically and empirically, that given certain assumptions and conditions being fulfilled, market outcomes tend to efficiency. So it is by no means the case that the debate between neoclassical and behavioral finance is decisively finished by the analysis presented here.

## 3.7 CONCLUSION

What then can be concluded from the data? First of all, while the sample is small, it has become clear that academic excellence is no guarantee of real-world investing excellence. The mutual funds analyzed here, which share a strong academic pedigree, are by no means bad investments. In most cases they do better than the broad market benchmarks, but three of the four funds examined fail to distinguish themselves within the community of similar mutual funds: a particular academic alpha does not exist in those three cases[56]. Fuller & Thaler's Undiscovered Managers Behavioral Value Fund then is the exception rather than the rule.

Second, there is no conclusive evidence that investment management operations which are advertised as based on principles of behavioral finance do provide consistently and persistently superior returns when compared to investment management operations which are advertised as inspired by principles of neoclassical finance, or the other way around for that

55 The price-kernel based asset pricing framework provides an integrated approach to asset pricing. Specific cases such as CAPM, the Fama-French multi-factor model and the Black-Scholes option-pricing formula can be derived as specific cases. The core idea behind the model is the idea of a stochastic discount factor (SDF), the price of an asset being determined by the expected value of the expected discounted cash flows. Specific cases give specific SDFs. A SDF can very well be behavioral, or behaviorally influenced (Cochrane, 2005, Shefrin, 2008).

56 In chapter five the impact of theory on practice in finance is discussed in deep. While it is true that in the area of investment and asset pricing theory has profoundly affected practice, the lack of "academic" alpha is interesting in that there appears to be no evidence here of performative or counterperformative effects (see MacKenzie, 2006 and MacKenzie et al., 2007). In the case considered in this chapter intervention by theoreticians does not appear to materially affect the practice.

matter. The behaviorally oriented small cap fund does perform better than its neoclassical rival, but with regard to the large cap funds the opposite can be argued. Differences are more pronounced in the short term. In the long term, what differences there are within both categories of funds, using the more advanced performance measures such as alpha and Sharpe ratio are neither spectacular in size nor persistent enough. At least in the case of the funds analyzed here, there is no evidence that a basic tenet of neoclassical finance – systematically and persistently beating the market in terms of risk and return is not easy or even possible – does not hold.

# Chapter 4

## The Austrian Middle Ground in Finance

*In this chapter ideas out of Austrian economics, in particular Israel Kirzner's thoughts on the market process, are applied to the workings of, and theorizing about financial markets. I argue, both theoretically and by using illustrations out of the practice of financial markets, that a descriptive causal process approach such as the Austrian viewpoint provides, can be regarded as complementary to the more normative claims put forward by neoclassical and behavioral finance, actually bridging the divide between these two dominant strands of thought in finance.*

## 4.1 INTRODUCTION

Austrian economics comes in a range of subschools; opinions can vary widely and disagreements amongst them are significant, and sometimes even acerbic. My aim here is not to argue for one subschool or the other or propose an "Austrian" finance theory but rather to focus on some thoughts about markets, originating in the Austrian school of economics, which I claim are relevant for financial markets.

Despite all the differences of opinion and the disagreements among Austrians, however, one of the depictions in mainstream neoclassical economics where almost all Austrian economists take serious issue with, is the "false and misleading picture of real markets" (Kirzner, 1997). In particular Kirzner, one of the more moderate Austrians, has gone to great lengths to develop an account of the market process, which provides a substantive story of how market coordination is achieved, instead of the neoclassical postulation of equilibrium at all times. Kirzner, in his own words, offers a middle ground between the neoclassical view where coordination is already implied by the assumption of perfect knowledge, and a radical subjectivist view[57] that states that the amount of ignorance is so great that it puts coordination beyond reach. "Knowledge is not perfect; but neither is ignorance necessarily invincible. Equilibrium is indeed never attained, yet the market does exhibit powerful tendencies toward it" (Kirzner, 1992, 5).

It is my aim here to build upon this concept of a middle ground using Kirzner's ideas and concepts for finance, in particular the fierce debate that is conducted between neoclassical and behavioral finance regarding market efficiency.

Kirzner's criticism on the false and misleading picture of real markets will be taken as a starting point of analysis. The central notion in my analysis is arbitrage. Arbitrage is the mechanism that should make markets (in general, but in this case specifically financial markets) function well. It is a crucial concept in virtually any account of financial markets. Theory and practice of financial markets are characterized by a close proximity and entwinement: concepts, methods and tools travel back and forth (see Bernstein, 1992). Likewise arbitrage as a theoretical concept has its neighbour in the real realm of the financial markets. By means of examples from the practice I will show that what actually goes on in financial markets arbitrage displays an uncanny resemblance to the Austrian theory of market process. Real-life arbitrage is characterized by alertness, search, discovery, entrepreneurship, group interaction, learning and imitation, all in the presence of uncertainty and facilitated but also constrained by language.

57 Kizner specifically mentions Ludwig Lachmann and George Schackle as representatives of this view.

Having established the descriptive relevance of Austrian market process theory gives rise to reconsider some of the hot issues in the neoclassical versus behavioral debate. The first such issue is market efficiency. It would make sense to decouple this notion from the concept of equilibrium, in the light of a dynamic market process. Equilibrium ultimately means that supply and demand meet, leading to markets clearing. Financial markets continuously clear: every transaction has a buyer and a seller. Whether it is a trade at the market equilibrium is a separate matter. What really matters socially is that financial markets perform their allocative function and provide a level playing field, that there is some tendency towards optimality. Only then can they perform their function in society.

The second issue is that of rationality. The debate in finance can also be looked upon as a question about economic agents. Who prevails in financial markets: the few hyper-rational arbitrageurs or the herds of irrational "noise-traders"? In the spirit of Fritz Machlup, we should pay attention to semantics here: what do we mean by those typologies? In a world where dealing with radical (Knightian) uncertainty is manifest and the possibility of error is acknowledged, these are not pictures of actual individuals. I would rather suggest that terms like "arbitrageur", "noise-trader", but also "the entrepreneur" and "the labourer" are properties of all individuals. Actual people embody all these economic properties, granted to varying extents and in combination with varying skills, dispositions, and preferences (see also Knight, 1921). That is to say: each one of us all can turn out to be more or less rational in various instances in grappling with the uncertain world.

Thus there is a true middle ground to be established by using the Austrian insights. Neoclassical finance would need to restate its idea about what it means for a market to be efficient. The equilibrium can be an excellent concept for theorizing but need not be a relevant actual state, is a notion we already know from physics. Moreover, explicitly giving up the concept of equilibrium is a thought already entertained by quite a few prominent finance scholars. On the other hand, behavioral finance should reconsider to what extent its claims, based to a large extent on psychology and experiments, are relevant for the outcomes of the market process, especially in a world characterized by radical uncertainty. Methodological individualism need not go so far as to derive market outcomes out of a realistic picture of an actual living individual, whatever that may be. Both those in the neoclassical and behavioral corner would do well to take into account the insights of Kirzner, Mises and Hayek, of Knight and Machlup. At the same time, this discussion, being an applied account of Austrian economics may present challenges and issues for Austrian economics itself.

## 4.2 FINANCE & ARBITRAGE

Several authors (Bernstein, 1992, Varian, 1992) have argued that finance has been one of the success stories in post-World War II economics, in particular where it regards quantitative economics This is evidenced by a disproportionate number of Nobel Prizes and other awards obtained for finance endeavours, the large numbers of journals, congresses and the like, and in general by the firmly established and significant place it has within departments of economics and business sciences in academia. Its success also shows in everyday life: financial markets are an inextricable part of modern society. Advances in finance theory certainly have played a role in the acceptance and legitimatizing of financial markets[58].

Where does this success stem from? Especially given that finance has only developed into a distinct field of research since the 1950s and thus is a relatively young discipline. The proximity and entwining of theory and practice is probably important. On the one hand financial markets provide scientists with an extraordinary amount of data to work with. On the other hand theoretical tools and concepts have found their way, largely unmodified, into the equipment box of the practitioners. Option pricing theory, which is used every day by derivatives traders and bankers, is probably the best example but only one of many that can be given (see Bernstein, 1992; MacKenzie, 2006).

Another factor in the success of finance could lie in its multidisciplinary background. Up to the 1950s finance was hardly recognized and regarded as a subject of economics. And indeed many of those who would become leading scholars in finance did not have a background in economics; they came for example from mathematics, physics, engineering, French, law, statistics, and astronomy (ibid.) Also, much of the ground-breaking work was done outside of the economics faculties, in business schools, and even outside the university, at think-thanks such as the RAND Corporation, consulting firms such as Arthur D. Little, and banks, for instance Wells Fargo Bank (ibid.).

Still, I would deem finance as an economic sub-discipline because its subject matter is economic: the coordination of inter-temporal consumption and saving. And indeed academic economics has had a profound impact on finance. Finance theory has largely been the result of getting the best out of two traditional powerhouses in economics: the empirically inclined Chicago School and the theoreticians out of MIT.

58 MacKenzie (2006) goes so far in this respect to claim that some parts of finance theory have had a performative role: shaping and creating the reality it intends to describe; see chapter five for an elaborate analysis of his arguments.

Consider the achievements then: the finance theories that have made headlines. Following various historical accounts (Bernstein, 1992; Mehrling, 2005; MacKenzie, 2006), five strands jump out[59]. In more or less chronological order:

- The work on portfolio theory by Harry Markowitz (and to some extent James Tobin), delivering crucial insights on risk and return, resulting in the now familiar mean-variance analysis;
- William Sharpe's (an others') contribution resulting in the capital asset pricing model (CAPM)[60], extending Markowitz's results to a coherent account of asset prices in relation to the market as a whole;
- The propositions of Franco Modigliani and Merton Miller, telling the theoretical story that the capital structure of a firm is irrelevant for its market value;
- The efficient market hypothesis, linked to the names of Eugene Fama and Paul Samuelson, which states that one cannot beat the market;
- Option pricing theory, conceived by Fischer Black, Myron Scholes, and Robert C. Merton, which enables one to put a meaningful value on pretty much any financial asset.

What these theories have in common is that they are about financial instruments and financial markets[61]. They all assume that these markets function well and they do so for one reason: the arbitrage principle.

Stephen Ross sets off his 2005 book on neoclassical finance as follows: "(Chapter) One: No Arbitrage, The Fundamental Theorem of Finance". Arbitrage is a crucial concept in finance. It is the key assumption which underlies all the various parts of established theory. Arbitrage can be defined as "the simultaneous purchase and sale of the same, or essentially similar security in two different markets for advantageously different prices" (Sharpe and Alexander, 1990). The classical illustration of arbitrage is that one doesn't find $20 bills lying on the pavement, because somebody else would have picked them up already. Now arbitrage as such is as old as economics itself: one can encounter it in the writings of Adam Smith and Alfred Marshall. The Law of One Price, Purchasing Power Parity—these are all variations on the same theme (see also Harrison, 1997).

But the treatment of arbitrage in finance is a bit different. As Bernstein (1992) has noted, instead of it being a common feature of competitive markets, it has been elevated to the level of a driving force. Arbitrage is not absent because markets are competitive. Rather, it is the

59 As any historian of finance will tell, much of this work was preceded by Louis Bachelier's dissertation from 1900 "Theorie de la Speculation", which contained important pieces of what would later become established finance theory. His accomplishments, however would only be recognized some fifty years later. See Jovanovic, 2008 & 2018.

60 Jack Treynor and John Lintner need mentioning as well, when talking about CAPM.

61 There exist important interrelations between all these theories.

other way around: arbitrage enforces competitiveness on the market. Modigliani and Miller used this "novel application" (Varian, 1993) to prove their propositions. It has become known as the arbitrage-proof, and it has played a critical role in all subsequent theorizing in finance (see Varian, 1987, for a more elaborate discussion, including a formal exposition). As Harrison (1997) puts it: "The successful application of economic theory in finance must be attributed to the notion of arbitrage. Not only could something "scientific" be said about speculative market prices, but also the economics theory seemed able to explain reality. This made finance all the more palatable to economists. Arbitrage was the theoretical force behind each of the major economics innovations in finance". And in the words of sociologist of science Donald MacKenzie (2003): "Arbitrage is the key theoretical mechanism in financial economics. A whole set of central propositions have been demonstrated by 'arbitrage-proof – the demonstration that if the proposition did not hold, then an arbitrage opportunity would open up".

This has certain implications, which makes finance distinct from (neoclassical) economics in general[62]. As Ross (2005) said: "with its emphasis on the absence of arbitrage, neoclassical finance takes a step back from the requirement that the markets be in full equilibrium". The No Arbitrage theorem gives an explanation how equilibrium will come about. While that may be regarded as a positive, it also raises new questions. Where does arbitrage come from, and how does it work?

Ross provided an answer to the first question: the no arbitrage assumption is based on "the most basic beliefs about human behavior, namely that there is someone who prefers having more wealth to having less" (ibid.). Behavioral finance has challenged this axiom. Not the fact that in general there is someone who prefers more to less, but based on insights from individual and social psychology, they take offense to the simplistic, rational picture of human agency is rejected, resulting in doubts about market efficiency.

The second question –how does arbitrage actually take place—is neither answered by neoclassical finance nor by the behavioral variety. MacKenzie (2003) remarked correctly that arbitrage in finance is a black box. I propose a substantive content for that black box by using the insights of Austrian Market Process Theory and looking at the practice of financial markets.

62 See also chapter 2.

## 4.3 MARKET PROCESS THEORY

Austrians may applaud Ross's "step back". In the words of Kirzner (1997): "At the market level, Austrians have rebelled against a microeconomics which can find coherence in markets and can explain market phenomena only by asserting that markets are, at all times, to be treated as if already in the attained state of equilibrium". But it is a hollow victory if equilibrium is still the unavoidable outcome of the assumption of no arbitrage, based on such a simplistic portrayal of human behavior. Indeed, according to Kirzner (ibid.), the second part of Austrian criticism of the unrealistic character of neoclassical economics is that "Austrians have taken sharp exception to the manner in which neoclassical theory has portrayed the individual decision as a mechanical exercise in constrained maximization. Such portrayal robs human choice of its essentially open-ended character, in which imagination and boldness must inevitably play a role".

The reason for that open-ended character lies in the fundamental uncertain nature of the economic domain; a point raised by Frank Knight (1921) that has been acknowledged by most of the Austrian economists[63]. Knight makes a threefold distinction (1921, pp. 224-225). First, there exist a priori probabilities: absolute objective chances like those in throwing a fair die. Second, there are statistical probabilities: objective, empirical evaluations of frequency of association. And third, there are estimates: subjective, more or less educated guesses, liable to error. The first two fall under the heading of risk. When we speak of risk, the distribution is known, either a priori or through empirical work, and we can obtain objective, measurable numbers. The third category is that of true radical, non-measurable uncertainty.

According to Knight, it is this third category which characterizes the economic domain in reality ("business" in his own words) and which gives rise to the phenomenon of profit (and loss). The reason is imperfect knowledge of the future. Knight also takes issue with the omniscient approach to agency in classical economics. What we can say about knowledge and behavior is that consciousness or awareness gives us the possibility of anticipating an image of the future. This image is always subjective: "We perceive the world before we react to it, and we react not to what we perceive, but always to what we infer" (1921, p. 201). Given this subjective character, it makes sense that this process is not infallible: "we do not perceive as it is, nor do we infer precisely, nor do we act knowing the consequences, nor do we execute perfectly" (1921, p. 202).

It is important to stress that acknowledging the existence of Knightian uncertainty does not imply that we are constantly subjected to the whims of chance. There is nothing mysterious

63 Nonetheless Kirzner (2000) has pointed out important differences between Knight and his ("Old") Chicago School disciples and the Austrian School.

or metaphysical about it, nor does it imply some utter wild randomness. Uncertainty simply is a consequence of the dynamics of real life, subject to change, disturbances, and surprises, and human nature, with its cognitive limitations and non-rational factors such as emotions and intuitions. Moreover, there are ways to deal with uncertainty: it can be reduced, which is the goal of rational conduct, but not eliminated (Knight, 1921, p. 238). Put differently by Kirzner (1992), our future is unpredictable because it is not a simple extrapolation of the past, but partially constructed by our present decisions and the decisions of others. In the words of Knight: "The existence of the problem of knowledge depends on the future being different from the past, but the possibility of the solution depends on the future being like the past" (1921 p. 313).

The key to dealing with uncertainty in the economic domain lies in the process of entrepreneurial discovery. Kirzner (1997) explicitly roots entrepreneurial discovery in the ideas of the great Austrian economists Ludwig von Mises and Friedrich Hayek. Specifically, Mises provides the view of the market as an entrepreneurially driven process, while Hayek provides insights into the role of knowledge and the coordinating role of the market in spreading and enhancing knowledge. Ignorance and discovery are the central characteristics of the market process. Ignorance—or differently put a lack of omniscience—will prevent equilibrium, while discovery will give rise to a tendency towards equilibrium.

Ignorance is not fully remediable by deliberate search and learning. Ignorance is an essential part of a world in which we have to deal with radical uncertainty, in the Knightian sense. Often we are hardly aware of our ignorance. Likewise, discovery is not to be equated with deliberate search and learning; there is always an element of surprise in it. Moreover, all real discovery is unplanned and thus to a large extent unintended. Not only has discovery an unpredictable element and is it somehow accidental, the results and consequences of it are also not fully predictable. Our future is partly constructed and determined by the choices we make. That renders the market process indeterminate and the market certainly not in a state of equilibrium at all times. But neither is it entirely directionless, in Kirzner's opinion, because of entrepreneurship[64].

Entrepreneurs are constantly on the lookout for potential profit opportunities. Their alertness to such opportunities leads them to action in order to grasp such opportunities. Results of such action are by no means assured, as is to be expected in a dynamic context of radical uncertainty: fallibility is acknowledged, errors are possible[65]. If, however, the entrepreneur succeeds in grasping a pure profit opportunity, dynamic competition will ensure that it is short-lived. Thus the power of the equilibrating process can vary greatly, as does the time

64 Radical subjectivists argue that uncertainty is so omnipresent that it is in fact directionless.

65 Joseph Schumpeter's "creative destruction" is illustrative of this line of thinking.

period it takes for an equilibrating move to appear. Empirically, though, it would seem that the equilibrating tendency is somehow there, even in extreme cases such as the 2007-2008 crisis.

When Peter Bernstein (1992) talked about arbitrage as the driving force of the market, one who is familiar with Austrian economics cannot fail to notice the resemblance to Kirzner's work, in particular his 2000 collection of essays, titled "The Driving Force of the Market". Austrians have typically limited their applied work to cases such as welfare economics and policy issues. Thinking of the market as a process would seem to be more like a state of mind or a broad conceptual frame than an actual description. But in the case of the workings of financial markets a quite narrow application seems possible: Austrian market process theory as a blueprint for arbitrage in financial markets.

A note of caution is in order here. Kirzner himself has said that "entrepreneurship cannot be reduced to any kind of arbitrage, because alertness does not remove all ignorance" (1979). That means that unnoticed profit opportunities will remain. It should be obvious that when Kirzner mentions "arbitrage" here, he means the academic variety: the instantaneous working of the universal Law of One Price. My comparison is about the actual arbitrage process in financial markets and Austrian market process theory. Indeed, Kirzner later states that "alertness is a concept, sufficiently elastic to cover not only the existence of existing arbitrage opportunities, but also the perception of inter-temporal speculative opportunities" (quoted by Binenbaum, 1995).

## 4.4 ARBITRAGE IN REAL LIFE

Arbitrage as a theoretical construct is by definition risk free. In reality there is no such thing. Practical arbitrage in financial markets always involves some degree of risk or uncertainty in the form of inter-temporality and a less than hundred percent similarity between securities. To illustrate what the process in financial markets entails, let me provide an example out of actual practice, which will bring a number of aspects of arbitrage to the front. The example is probably the most risk-free arbitrage opportunity that I have encountered in my own experience in the markets[66].



66 The only alteration from the deal as it happened is the stock symbol.

**Example 1:** real-life arbitrage

EIPE is a huge, publicly traded company, a global player with one of the biggest market capitalizations in the world. Its shares are primarily listed on the New York Stock Exchange. Options on EIPE shares are one of the most traded option classes in the United States: liquidity is extremely high and the bid-ask spread is minimal.

Securities markets, while widely accessible for people all over the world, are still characterized by certain barriers to transactions. For instance, foreigners often have to go through certain formalities, and incur costs to be able to trade overseas. With this in mind, the Amsterdam Option Exchange in 2000 decides that there might be interest amongst its clients and investors to trade options on EIPE shares on the Amsterdam exchange. Options on EIPE shares are listed in the Netherlands and a market is continually made in the securities[67]. Attention from the investing public, however, turns out being almost non-existent.

In December 2002 a professional trader takes a peek at these options and decides to compare the prices with those quoted in the US markets[68]. She is quite surprised to observe the following (all quotes in US $): [69]

| | EIPE NL | EIPE US |
|---|---|---|
| C apr 45 | 1.60-1.90 | 1.20-1.30 |
| C apr 50 | 1.00-1.30 | 0.70-0.80 |
| C apr 55 | 0.60-0.90 | 0.40-0.50 |
| C jul 45 | 2.50-3.00 | 2.00-2.10 |
| C jul 50 | 1.40-1.90 | 1.10-1.20 |
| C jul 55 | 0.90-1.20 | 0.80-0.90 |

She could purchase an EIPE C apr 45 in the US for $1.30 and sell it in the Netherlands for $1.60, creating an immediate profit of $30 per option contract[70]. How is this possible: two seemingly identical objects having vastly different prices? Perhaps, she pondered, they are not identical. A thorough inspection was done, including analysis by the risk management

67 A market being made means that there are continuous pricing quotes on which one can trade.

68 Overseas arbitrage is common practice in the financial markets: many shares have multiple listings on various exchanges in the world and simple algorithms are usually utilized to monitor price differences. In option markets this is less common and much more complicated.

69 C apr 45 denotes a call option on 100 shares of EIPE, expiring in April. 1.60-1.90 means that one will have to pay $1.90 to purchase the option and one will receive $1.60 to sell the option.

70 One call option gives the owner the right to purchase 100 shares, so the gain would be $0.30 multiplied by 100.

department, but nothing could be found; both securities had exactly the same specifications. So, was this a risk-free arbitrage opportunity?

One caveat showed up: exchanges are linked to clearing organizations. A clearing organization is essentially the warehouse of an exchange. It facilitates trading, arranging administrative matters such as transfers of money and securities between parties. In order to trade securities one needs to put up capital at the clearing organization, both for transaction purposes (liquidity) and as a guarantee against possible losses (solvency). Different exchanges have different clearing organizations, which usually are not linked to each other. In this case that meant that, even while the trade itself was entirely risk-free, capital had to be deposited at both the Dutch and the American clearing organizations until the position would unwind on the expiration date of the options. This has an important implication: because costs are incurred in setting up the position, certain trades are not profitable enough, given cost of capital and return on investment requirements. For instance, the $0.10 gain on EIPE C apr 55 and the $0.20 gain on EIPE C jul 50 were not enough to offset these constraints[71].

But other trades could be executed with enough profitability and so it was done. In three days 5000 options were bought and sold for an average net gain of $0.20, resulting in a $100,000 profit. After three days the opportunities disappeared; prices in the Netherlands and the United States now were in line with each other again.

What does this example tell us about the arbitrage mechanism? First of all, arbitrage opportunities are not obvious or easy to spot and execute. One has to be alert on spotting them, digging in the vast universe of financial products. Moreover, finding such an opportunity is indeed surprising, especially given the extremely high degree of similarity between the two securities in the example.

Secondly, arbitrage takes time: time for search and investigating the opportunity, time for execution, but also time until the gain is realized[72]. In fact it is entirely possible that the opportunity becomes even more advantageous. So the process is not only dynamic instead of static with instantaneous adjustments, but it is also to some extent indeterminate.

Thirdly, capital is required. As in textbook arbitrage—risk-free by definition—nothing needs to be said about risk attitudes or preferences in the example. A form of uncertainty presents

71 July options, because of their later date of expiration, require a higher return in order to produce the same annualized return on investment.

72 In this example, the full gain was ultimately realized when the options expired.

itself in the capital constraint: one might be forced for some reason to prematurely liquidate a position before the gain can be realized[73].

Fourthly, this is clearly a specialized activity, not accessible to just any economic agent. Access to information, means of execution, and capital (see the previous point) make such transactions the domain of a relatively small group of experts[74].

Fifthly, prices in this case are evidently subjective. It is not about one or the other price being too high or too low. Nothing needs to be said about the fair or fundamental value of the two securities except that they should be equal to one other.

And finally, in the process, over time, information can be revealed by the transaction. Other market participants, in particular the counterparties, will become aware of the differences in prices. This will tend to eliminate the arbitrage opportunity.

All in all, this example of real world arbitrage would seem to follow the depiction of the market process of entrepreneurial discovery quite closely. We have the trader in the role of entrepreneur looking for potential profit opportunities. Discovery of such an opportunity indeed lies "midway between that of the deliberately produced information in standard search theory, and that of sheer windfall gain generated by pure chance" (Kirzner, 1997, p.72). And finally, in the process, driven by dynamic competition between entrepreneurs/arbitrageurs, information is revealed and propagated through the market, which leads to exhaustion of existing opportunities but perhaps also to the discovery of new ones.

What also becomes apparent is that real arbitrage is not like the story of the $20 bills lying on the pavement. There are constraints, the opportunity is clouded by uncertainty, and it is not a matter of pure chance that can befall just anyone.

## 4.5 THE POSSIBILITY OF A MIDDLE GROUND

Now, it is nice to have a descriptive account of the arbitrage mechanism, but what are the implications for established finance theory? Does the Austrian perspective change the way we should think and theorize about what actually goes on in financial markets and the debate between neoclassical finance and behavioral finance? I assert that it can do in an important

73 Shleifer and Vishny (1997) provide an excellent illustration of how capital constraints and a limited number of agents can influence arbitrage.

74 MacKenzie has made the point, that, since the actors are a selective community, processes of social interaction, for instance imitation, might play an important role in financial markets (in Knorr-Cetina and Preda, 2005).

way. It alters how we should reflect on market efficiency, the domain where neoclassical finance has made its strongest claims. And it also helps to get a better, more realistic picture, a deeper insight in what agency in financial markets entails, an issue typically emphasized in behavioral finance. Moreover, there is no need to plainly reject the typical neoclassical and behavioral claims in full.

But before I foray in the specifics of reconciling the various accounts a question should be asked. Aren't these theories so fundamentally opposed to each other and essentially different that they are mutually excluding? Efficient contra inefficient, rational versus irrational/bounded rational, these appear to be deep and profound differences of opinion. However, if we take a close look at the kinds of theories that we are dealing with, and see what essentially is claimed, it will become apparent that these apparent rivals can coexist.

Mäki (1992a) has argued that Austrian market process theory has the structure of a causal process theory in that it, first, provides an account of a process as a sequence of events, and, second, that it depicts the driving forces that set and keep the process in motion, i.e. the causes of the motion[75]. Entrepreneurship represents a form of causal agency. Alertness acts as a causal power in that, ignited by the possibility of profit, entrepreneurs perceive opportunities (which may or may not turn out to be real) and act with purpose on those perceptions[76]. But that only provides an account of the emergence of a process. We also need to specify what is produced and how the process is sustained. Mäki answers that question as well. In the Austrian market process the obvious candidate for the stuff that is produced and propagated is information –the Hayekian part of the market process in addition to the Misean part of entrepreneurship. Information is reflected in disequilibrium market prices which function as imperfect signals.

Causal process theories can be distinguished from ideal type theories. The latter are not so much concerned with realistic descriptions of the workings of the world but rather take the form of what the world would be like if certain conditions are fulfilled. These conditions can be axioms, assumptions, theorems and the like. In this regard Mäki contrasts Austrian theory with Walrasian general equilibrium theory where the assumption of complete information results in one equilibrium price which clears all markets. Neoclassical finance has a similar ideal type structure: given the absence of arbitrage, financial markets will turn out to be efficient. And likewise for behavorial finance: given the cognitive limitations of human agents, their behavior will not be in line with what rational choice theory dictates and will display systematic biases. But the fact that Austrian market process theory is a different kind of

75 For Mäki's writings on Austrian economics in connection to philosophy of science and methodology, see Mäki 1991a, 1991b, 1992a.

76 That can also mean not acting, i.e. declining the opportunity because of doubts about the feasibility.

theory then neoclassical and behavioral finance does not suffice for complementarity of the former with the latter two.

To establish complementarity we need another insight on economic theories. Mäki (1992a) also argues that Austrian theory is an isolative theory in that it only is concerned with a particular slice of economic reality; in this case the essence of the functioning of the market as a process of entrepreneurial discovery. It aims to depict "the fundamental driving forces or the causal powers and the tendencies they give rise to" (ibid.). Elsewhere Mäki has argued extensively that the method of isolation is ubiquitous in economics (in particular Mäki1992b) and that appears to apply to neoclassical and behavioral finance as well.

The earlier remarks about neoclassical and behavioral finance can serve to formulate an essence for both of them. Neoclassical finance is about the (supposedly efficient) equilibrium outcome of the market process as the result of the No Arbitrage theorem and the interaction of economic agents. It hasn't any specific claims to make about how that process actually takes place, or about the agents that are engaged in the process (cf. Ross, 2005). Similarly for behavioral finance where claims are made on the decision making of individual agents in financial matters. It is neither about the process which is comprised of multiple agents, nor about the aggregate outcomes of the interaction between those agents. These theories focus on distinct slices of economic reality. There is a theory about individual agents and there is another theory about market outcomes. And, as I propose, there is yet another theory which tells us something about how market outcomes come about: Austrian market process theory. However, there are tensions which need to be addressed.

## 4.6 AUSTRIAN VERSUS NEOCLASSICAL

The main tension between the neoclassical and Austrian point of view would appear to be situated around the concept of equilibrium. Equilibrium is an important aspect in finance: it plays an explicit role in a number of theories. It becomes truly apparent in the work of Modigliani and Miller on the capital structure of the firm. They use an arbitrage proof for their theorem that—under certain assumptions—the choice between debt and equity financing is irrelevant to the value of a firm. Profit-seeking investors will ensure that the risk/return trade-off of debt and equity instruments will be equal: an equilibrium situation. In the words of Merton Miller (1999) himself: "The M&M propositions… are about equilibrium in capital markets—what equilibrium looks like". And indeed, given the assumptions for frictionless, competitive markets, the arbitrage principle will enforce equilibrium in such markets.

Where equilibrium really takes centre stage is in asset pricing models, beginning with the Capital Asset Pricing Model (CAPM). CAPM tells what asset prices should be in relation to the market as a whole, i.e. their equilibrium value, depending only on an asset's covariance with the market (Varian, 1993). That covariance has become known as beta. In its original conception CAPM is a so-called partial equilibrium model: it only pertains to one specific market[77]. But a general equilibrium derivation is also possible[78].

In later developments more factors besides beta were incorporated leading to the so-called multifactor models where more state variables can enter the equation[79]. Stephen Ross, explicitly starting from a no-arbitrage argument, developed Arbitrage Pricing Theory, which is a partial equilibrium approach. Robert C. Merton at MIT came up with an intertemporal capital asset pricing model, using the concepts of continuous time analysis (see Merton, 1975). His approach has been labelled a full general equilibrium model, having its roots in the work of Kenneth Arrow and Gerard Debreu (Bernstein, 1992, Miller, 1999a).

For the purpose of this discussion the difference is best expressed in terms of relative and absolute prices. General equilibrium is a form of absolute pricing, while partial equilibrium can be interpreted as more of a relative pricing approach[80]. The latter would clearly sit better with the subjectivism of Austrian economics, dating back to the Marginalist revolution and Carl Menger. So the semantics, what we mean by equilibrium, matters. A general equilibrium portrayal with all markets at all times in a state of equilibrium seems less realistic—and even more objectionable for an Austrian—than the assumption of one market being in equilibrium.

But even when we look at a single market, say a market for one particular stock, characterized by thousands of transactions each day, what does "equilibrium" mean? The only notion that makes sense is that each and every one of the single transaction prices is an equilibrium price, bringing together supply and demand at that point in time. The formulation makes something abundantly clear: there is invariable change, equilibrium is virtually constantly shifting. Clearly then an aggregate analysis for the stock market as a whole in terms of static equilibrium is problematic.

That is admitted by almost everybody. Yet such a description of capital markets is a keystone of finance. The Efficient Market Hypothesis comes in many guises (Lo and MacKinlay, 1999) but essentially, in Eugene Fama's own words (1970), it boils down to the idea that

77 Conform the Marshallian stance of the Chicago School.
78 Much depends on what one comprises under the phrase "market portfolio".
79 See for instance Cochrane (2001), for an exhaustive survey of asset pricing.
80 Although there are partial equilibrium models which provide absolute prices, like the Consumption Capital Asset Pricing Model (CCAPM).

"prices fully reflect all available information". That means that there is no possibility of outperforming the market, that there is instantaneous adjustment to new information, that there exist no arbitrage opportunities. In other words: a state of equilibrium and the omission of a process that would lead to such a state.

Finance, in the words of Perry Mehrling (2005), is about time, risk, and uncertainty: matching assets and liabilities to deal with the various hands that we are dealt with in life. The distinction between risk (quantifiable) and uncertainty (unquantifiable) is an important one. Most finance scholars, and economists in general, seem to be aware of the distinction, if we go by some of their words. But after acknowledging the existence of uncertainty, not much is said about it anymore. Through simplifying assumptions the emphasis in the analysis typically shifts to a quantifiable approach of the problem (see for example Merton, 1975).

The simplification is not a problem per se; in fact, that is part of doing science. But that does not make uncertainty disappear. In the social sciences in general, and in financial markets in particular, we would be well advised not to forget about it. Bernstein (1996) notes that "Knight's ideas are particularly relevant to financial markets where all decisions reflect a forecast of the future and where surprise occurs regularly". For Austrians the main reason for rejecting equilibrium reasoning lies in the recognition of radical, Knightian uncertainty.

**Example 2:** radical uncertainty within markets

Until approximately fifteen years ago Royal Dutch/Shell and Unilever were so-called Anglo-Dutch companies. Half of their shares were listed on the London Stock Exchange in the United Kingdom and half on the Amsterdam Stock Exchange in the Netherlands. A naïve account would suggest that both stocks represent the same value, of course adjusted for the different currencies. After all they represent the same piece of ownership, the same claim on future discounted cash flows. But in reality they never trade at the same prices. Now there are plenty of good reasons why they don't: fiscal regimes, market liquidity, institutional investment structures all differ between the United Kingdom and the Netherlands. Still, for trading purposes the dual shares should share a great degree of similarity. And they usually do. The way most arbitrageurs would trade on this similarity is by watching the spread between the Dutch and the British shares and apply statistics to decide which share to buy and which to sell short. The moving average would serve as point of reference and the spread would be put on and taken of according to the number of standard deviations that the spread was moving away from the mean. Note that use of a moving average provides a built-in safeguard for making sure that the position cannot easily get out of hand when the spread keeps widening and liquidity constraints force elimination of the position. It implicitly assumes a form of continuity in the price movement of both shares: gradual movements.

Because of the fundamental relationship and its supposed logical predictability, the position could be quite large, using considerable leverage as is usual in arbitrage trading.

Being among the largest companies in the world the Dutch shares of Royal Dutch/Shell and Unilever were also included in Standards & Poor's 500 Index, the leading American index for large cap stocks. In July of 2002 Standards & Poor's decided it wanted the S&P 500 index to consist only of American companies: the non-US companies Royal Dutch/Shell and Unilever would be replaced by domestic ones. The announcement came as a surprise to the market and led to mayhem. The share prices of both Dutch shares gapped (a discontinuous jump) and this set the spread with the British shares suddenly up to unprecedented levels. The S&P 500 was the leading index and as such the leading benchmark for many fund managers. Their performance is measured by comparison to the index and in many cases their investment policy was, or had to be, investing by following the index. The consequence of removal of the Dutch shares was that fund managers had to remove them as well from their portfolios: enormous amounts of Dutch shares had to be sold by the mutual funds. Moreover, they had to be sold quickly in order to keep tracking the index. While some may have gotten lucky by being short Dutch shares and long British ones, many arbitrageurs were caught in a trap. Moreover, with the spread getting so far out of line and leverage being used, some were forced to liquidate their positions at the most negative of points, exacerbating the situation[81]. A situation arose where at the same time while people were liquidating their positions taking huge losses, the profit opportunity was unprecedented. This can hardly be regarded as a stable, equilibrium situation. In time, the spread gradually moved back to more normal levels (which is only logical if the trading strategy remained somewhat identical)[82].

Could the arbitrage traders have foreseen the decision to remove the shares from the S&P 500, a decision taken by policy makers and which had nothing to do with (beliefs about) the underlying performance of the companies? Judging from the market reactions the market hadn't anticipated such an event. So it did neither enter the trading model, nor the risk management models.

Acknowledging uncertainty sheds light on various issues in finance. If we accept Knight's claim that economic life, "business", is characterized by uncertainty, then so should asset prices. Market prices are estimates. Covariances of assets with the market portfolio are estimates as well for the simple reason that the market portfolio is not a well-defined concept[83]. Furthermore, covariances are about past behavior and as Harry Markowitz already noted

81 Along the lines of what Shleifer and Vishny (1997) show.
82 This arbitrage trade is actually still employed by hedge funds.
83 This is the point of the so-called Roll critique: the market portfolio is an ambiguous concept because theoretically it should comprise any asset that has value, including such non-measurable items as human capital and goodwill (Roll, 1977).

in the 1950s, past experience is unlikely to be a very good guide to future performance (Bernstein, 1992, p.63).

In a sense the Efficient Market Hypothesis does justice to the consequences of uncertainty: the current price is the best estimate, rendering prediction useless. The Efficient Market Hypothesis, also known as the Random Walk, is what has given rise to the big question of financial markets: can one, consistently, beat the market? The evidence is mixed, depending on method, time-frame, data massaging and some insurmountable methodological issues[84] [85]. Fama himself acknowledges that "like all models, market efficiency…is a faulty description of price information" (1998) and in time he has adjusted and refined his opinion somewhat. His claim, however, remains that in general nothing fits better –in terms of statistical significance—and that critics, mostly coming from behavioral finance, have not been able to come up with a more convincing model.

But in a way the efficient markets argument excludes the possibility of error and denies the dynamic character of price formation in financial markets. The point becomes sharply clear when a translation is made from the Efficient Market Hypothesis into the assumption of a lognormal distribution for asset prices, which is quasi-universal in theorizing in finance. It is a perfect example of transforming uncertainty into risk. When the distribution is known, either a priori or through empirical results, we speak of risk, just like Knight meant it. But the lognormal distribution is not the distribution that is observed empirically (see Bernstein, 1992). Real distributions typically display so-called fat tails, i.e. a higher frequency of extreme values. These types of distribution are problematic in theorizing and model building, which makes it convenient to use the lognormal distribution[86]. The step of transforming uncertainty into risk can be defended and is common in other areas of economics and abstraction and idealization are part of doing economics and science in general (see Mäki, 1992b). But by adding subjective estimates to objective risk, the meaning of "risk" changes and is no longer in line with Knight's definition. In making claims about the real world we should keep in mind that we are also dealing with uncertainty, besides objectifiable risk, and that what is actually empirically observed is a distribution with fat tails.

84 There is an extensive amount of empirical work on market efficiency, see for example Jensen (1978), Malkiel (2003), Shiller (2003).

85 The Efficient Market Hypothesis is a joint hypothesis. Tests of market efficiency always imply simultaneously the test of an asset pricing model such as CAPM.

86 "Fat tails" are characteristic of infinite variance distributions. These distributions prohibit the mean-variance analysis which is so typical for much economic and econometric work. The possible accuracy of infinite variance distributions, as put forward by Benoit Mandelbrot, raised widespread concerns amongst economists over the validity of their results, which were based on normal distributions. The issue was settled in a rather pragmatic way by deciding that the assumption of normality was the best workable hypothesis (Mehrling, 2005; MacKenzie, 2006).

Something similar applies to Option Pricing Theory. Option Pricing Theory is a theory that actually deals with the open-endedness which is typical of many situations in the real world. Black, Scholes, and Merton developed tools to put a price tag on uncertainty. Derivatives derive their value from another asset: they are contingent claims. It is obvious that derivatives can only have value if there is uncertainty surrounding the underlying asset. The key to Option Pricing Theory is the volatility of the underlying asset, measured by its standard deviation. Unfortunately, volatility is an unpredictable entity: we can only determine afterwards how much movement has occurred. That means that volatility is uncertain, as has been acknowledged by Black: "I am unwilling to write down any definitive model of the general process by which volatilities change, and then use statistical methods to estimate the numbers that appear in the model" (in Mehrling, 2005). In other words, Black refused to transform volatility from an uncertain entity into a risky one and recognized the difference between the two.

The problem in finance is also methodological. As was shown in chapter 2, the method of argumentation in finance, in both the neoclassical and behavioral varieties is usually a narrow statistical treatment of empirical data. Methodologically it is positivist in the particular sense of Milton Friedman's 1953 essay "The Methodology of Positive Economics" (Miller, 1999b). Now there is nothing wrong with good empirical work and proper use of statistics therein[87]. But there is a sharp distinction between objective probabilities and subjective estimates. The former is not subjected to uncertainty and applies to the future as it does to the present and has done to the past. The latter is tentative; an educated guess for present and future, based, among others, on past experience but not exclusively so. Most finance research clearly is concerned with the domain of subjective estimates, but is not explicitly presented as such.[88]

In this regard, one may sympathize with the Austrians in their distrust of quantitative methods and formal modelling. Because those methods do not justice to the substantive content of the market process, and because of the frequent inappropriateness of an exclusively quantitative approach. The Austrian School has been commended for its attention to methodology, compared to other strands of economics (see Hands, 2001). Finance never has been bothered too much by methodological considerations. In chapter two Merton Miller was quoted: "the profession, from the outset, wholeheartedly adopted the Friedman positivist view: that what counts is not the literal accuracy of the assumptions but the predictions of the model" (1999b). Predictions are either borne out by the facts or not. But in the face of radical uncertainty awareness of this and some modesty about predictions should be advisable.

87 see McCloskey, 1994, 1998, and especially 1996 and McCloskey & Ziliak, 2008

88 This applies as well to much of mainstream economics, when using Savage's expected utility theory.

I propose that the Austrian market process account, which tells a story of price discovery, knowledge, and information and how people deal with those things, can supplement the claim made by the Efficient Market Hypothesis. The equilibrating tendency of the market process is a better description of what is going on in actual markets. Indeed, many of the finest finance scholars have acknowledged this. Fischer Black again serves as an excellent example. Black was a firm admirer and believer of the equilibrium concept as there ever was one; reportedly he even tried to apply CAPM to various aspects of his life (Mehrling, 2005). But he recognized the difference between value and price and he admitted that what actually happens in markets is a different story[89]:

> An efficient market is one in which price is within a factor 2 of value, i.e. the price is more than half of value and less than twice value… The factor of 2 is arbitrary, of course. Intuitively, though, it seems reasonable to me, in the light of sources of uncertainty about value and the strength of the forces tending to cause price to return to value. By this definition, I think almost all markets are efficient almost all of the time. 'Almost all' means at least 90%" (Black, 1986).

And further back in time, for James Tobin it was his discomfort with the highly restrictive assumption of complete equilibrium at all times that led him towards the Separation Theorem[90].

More recently, others have explicitly suggested leaving the equilibrium concept behind us (for instance Campbell, 2000). Interesting in this regard from an Austrian point of view is the work of Harald Benink et al. (Benink and Bossaerts (2001), Benink, Gordillo, Pardo and Stephens, (2010)). They label their approach Neo-Austrian, referring to Hayek's insight on knowledge and learning, and Kirzner's entrepreneurial discovery process. At the same point Benink et al. apply familiar tools of finance theorizing: formal model building, simulation, statistics. This might be objectionable to some in the Austrian School, but it does provide an illustrative way of comparing an Austrian-type perspective to the neoclassical one: it shows what disequilibrium—with an equilibrating tendency—looks like.

What it looks like is a "process…stable yet continuously featuring inefficiencies, keeping the market from reaching its fully efficient equilibrium" (2001). Investors are not completely ignorant, but their knowledge is limited: "(they) are unable to exploit all inefficiencies because they cannot make reliable inferences" (2001). Their conclusion is that "if inefficiencies

89 Fischer Black, of course, was not solely an academic, having spent a considerable part of his career on Wall Street at Goldman Sachs.

90 Tobin's Separation Theorem states that the selection process for a risky efficient portfolio is completely separate from the portfolio allocation decision between risky and risk-free assets (Tobin, 1958).

are of the neo-Austrian kind, not much can be said beyond admitting that inefficiencies exist". Benink and Bossaerts present this as a paradox, but is it really? It is not when one acknowledges the existence and presence of radical uncertainty, the kind of uncertainty that causes disturbances and forces us to grapple with reality.

That is exactly what an Austrian would argue. Thus the findings of Benink and Bossaerts could be regarded as evidence of the validity of Austrian insights in the market process. They are also of importance for the debate between neoclassical and behavioral finance in that they show that market inefficiency is not necessarily to be equated with opportunities to beat the market in a consistent fashion. The keyword here is "consistent". It is one thing to state that a market is not fully efficient and another thing to claim that there exist systematic tendencies in that market which can be regularly exploited.

That takes a bite out of some of the behavioral criticism, in particular from Richard Thaler and his followers who claim that there is money to be made from the so-called anomalies (see for instance DeBondt and Thaler, 1986 and the previous chapter)[91]. Others, like Robert Shiller (1989) and Stephen LeRoy (1989), seem to claim only inefficiency, not opportunities to make money. The market displays too much volatility to be deemed efficient, but is also too volatile and unpredictable to make easy money. The latter position seems reconcilable with the Austrian perspective of disequilibrium.

On the other side one cannot fail to notice the similarities between the Efficient Market Hypothesis and Hayek's thought on knowledge and learning. Both are about information and price formation, about discovery and dispersal of knowledge through the market. The Efficient Market Hypothesis as such is clearly not a realistic proposition. It highlights the link between information and price formation but supposes that people translate information unambiguously and instantaneously in prices. The Hayekian insights take a step back from the idealized and unrealistic picture of efficient markets, but can be regarded as complementary, providing content to the process. Hayek and the Austrians give us a handle on how actual prices come about, what they accomplish, and in general provide an account of the coordinating properties of the market.

Quite a bit has been written on the links between Hayek and neoclassical economics, in particular the Chicago School. Recently Colin-Jaeger & Delcey (2019) took this one step further towards finance by investigating the similarities between Hayek and Eugene Fama. They conclude that, despite methodological differences, the theory of prices, based on information, of Fama and Hayek is epistemologically similar, as is their conception of efficiency.

91 See chapter 3 for a performance test of neoclassical and behavioral investing.

In other words, they share common understanding of how markets, competition and prices work based on a common understanding of the notion of information. "This common understanding reveals a common representation of the interaction between individuals and then an answer to the problem of coordination" and "rationality is consequently not a hypothesis but the result of competition", according to Colin-Jaeger and Delcey (ibid.). Furthermore, they contend that Hayek underlines the entrepreneurial process of the competition, with reference to Kirzner and that traders may be seen as (Hayekian) entrepreneurs.

Finally, as was shown in the example, the market process, with its focus on alert entrepreneurs looking for potential profit opportunities provides an account of the dynamics that drive the market towards efficiency, towards equilibrium. One of the attractions of equilibrium theorizing is the identification of optima: states where some criterion is maximized. But not much is sacrificed if we take an Austrian non-equilibrium stance. The outcome from the preceding analysis is still a preferable one from a social perspective: a fair, reasonably well functioning market, tending towards optimality.

Does this render the equilibrium concept entirely useless, as is claimed, for instance, by Ludwig Lachmann? I don't think so, and neither do Kirzner and Hayek (see Kirzner, 1992). It is clear that in finance equilibrium theorizing has produced impressive results[92]. Neoclassical and Austrian analysis, both rooted in the Marginalist Revolution, analysis can coexist, depending on the questions asked and what it is that we want to explain –the explananda.

## 4.7 AUSTRIAN VERSUS BEHAVIORAL

It would appear that behavioral finance would be in a better position to deal with uncertainty. After all, their starting point is decision making under uncertainty. Behavioral economists have in common an emphasis on cognitive limitations and the role of perception, intuition, reflexivity, and the use of heuristics in complex situations. They do recognize the troubles of individuals finding their way in an uncertain world. However, the way behavioral economists treat decision making does not differ much from their neoclassical colleagues by the transformation of (Knightean) uncertainty into risk.

Behavioral finance commonly backs up its claims with experiments. Typically in experiments the environment is controlled, the goal being to isolate certain aspects. Which is common practice in science. But what is the value of the outcomes of such experiments, done in a closed setting, for the open reality? The question, also known as the problem of external

92 Perhaps we should think of an analogy with the Second Law of Thermodynamics, which defines the equilibrium condition, yet also states that it will never hold.

validity, is particularly relevant when uncertainty is considered. After all, it is precisely the dynamic, complex character of real life that causes uncertainty in the first place. Experiments are typically framed as choices between cut-and-dried alternatives. Let's suppose it involves the choice between receiving $20 today or $25 tomorrow. The idea is that we can infer something about time-preference here. It would seem like a good idea to wait for a day and receive $5 more. But who is to say that, when I choose the first option of $20 today, I won't receive some other amount tomorrow? Those kinds of possibilities are typically excluded. Experiments might not be very appropriate for situations where uncertainty is prevalent, as for instance in financial markets.

Another issue is the claim that there exist systematic anomalies in financial markets caused by less than rational investor behavior, that there are irrational acts we all commit. For instance, in one of the earlier contributions in behavioral finance, "Does the Stock Market Overreact?" (1985) DeBondt and Thaler claim that stock prices systematically overshoot (both up and down) upon the arrival of new information. This is due to investors updating their beliefs in a non-Bayesian manner: more recent information is overweighed. From an Austrian point of view, that result is perfectly defensible. Kirzner has repeatedly (1992, 1997, 2000) pointed out that, in the face of the imperfection of knowledge and the element of surprise, people are frequently overly optimistic or pessimistic. Such perceptions and the resulting mistakes are essential parts of the make-up of human agents, entrepreneurs in particular. But "although entrepreneurs make errors, there is no tendency for entrepreneurial errors to be made" (Kirzner, 1997). That leaves the behavioral claim just as deficient as the standard neoclassical picture of the omniscient rational maximizer. Uncertainty entails that every situation is unique and that people's actions and reactions are not uniform (Knight, 1921). Any grand claim about systematic behavior therefore denies the indeterminateness of life caused by uncertainty.

Still, one of the biggest issues in the debate between neoclassical and behavioral finance concerns the rationality of investors. Most of us simply do not behave according to the normative model of rational decision-making, as has been made abundantly clear by Daniel Kahneman, Amos Tversky, Richard Thaler and others. They take issue with the assumption that "only rational behavior can survive in a competitive environment" (Kahneman and Tversky, 1979).

Financial markets are (or are supposed to be) vigorously competitive. So is the irrationality, which belongs to our psychological make-up present and visible in the market, as has been suggested by Thaler but also much earlier by Keynes (see Raines and Leathers, 2000)? The neoclassical reply is the idea of "a few good men": "neoclassical finance is a theory of sharks (i.e. arbitrageurs), not of rational homo economicus" (Ross, 2005). As was said before:

arbitrage is the driving force in financial markets and for arbitrage to work there only need to be someone who prefers more over less. In other words, there is no need for a universal rationality assumption. In the words of Ross: "I, for one, never thought that people—myself included—are all that rational in their behavior. To the contrary, I am always amazed at what people do. But that was never the point of financial theory" (ibid.).

The point is that in a highly competitive market environment suboptimal behavior should be weeded out. Despite people constantly behaving "irrational", the market outcome can be pretty efficient, as has been forcefully argued by many, for instance Merton Miller (1987) and Vernon Smith (1989). That is also a conclusion of the Kirznerian Austrians. In general, Hayek (1967) notes, "that the systems of rules of individual conduct and the order of actions which results from the individuals acting in accordance with them are not the same thing". For Austrians the economic issue par excellence is the aggregation and coordination question. Despite the method of praxeology, their focus lies not so much on the actions of individuals, but on the consequences of the interaction between individuals.

One important question remains: who are these arbitrageurs/entrepreneurs who make it right? What is this "smart money"? If we are all subject to uncertainty and an open-ended future with which we have to deal with our cognitive limitations and "irrational" inhibitions, how can we expect that there are people who constantly make optimal decisions? I believe it rather unlikely to encounter such people; in fact I'll posit that there aren't any such living persons.

The answer to the question lies in separating real human individuals from agents in financial markets. There is no such thing as someone being a one hundred percent entrepreneur or a one hundred percent speculator. Both aspects are present in real people. Heterogeneity of agents resides within actors, not between them. Mises has said it correctly: "in any real and living economy, every actor is always an entrepreneur and a speculator" (quoted by Wubben, 1995). In finance a similar distinction has been made between informed traders, arbitrageurs who make the most out of information, and noise traders, who might believe they act on genuine information but are actually not doing so (see Black, 1986).

Noise can actually be regarded as the main reason why we observe so much trading in financial markets: people having different opinions and acting on those varying opinions. However, it is not so easy to distinguish between noise and "true" information. It is only ex-post that we can determine who had it right. In fact, an investor can be both noise trader and informed trader at the same time: he can be right about one investment idea while being wrong on another count. He can be noisy one day, informed the next. We simply cannot tell in advance. It is only in the dynamic market process that true information is revealed, that

noise is filtered out and that we can determine whether one has been a successful entrepreneur or a speculator.

The actual practice of professional investing is an effective illustration of this. There aren't any investment managers with a track record of consistent outperformance. Even highly successful and well-known names, such as George Soros and Warren Buffett, have taken their lumps occasionally. In fact, there is evidence that the performance of money managers is mean-reverting: outperformance in one time period increases the likelihood of underperformance in the next period (Bernstein, 1992).

That does shred some doubt on the behavioral claim. If investment performance is not traceable to individual human behavior, but rather the result of "pervasive market forces" (Miller, 1987), how much do individual characteristics matter? Moreover, if all our behavior is tentative, while still being deliberate and purposeful, an element of randomness enters the equation inevitably. In this way speculation, so often considered as undesirable and damaging to society, loses that negative connotation, as it also does in the writings of Mises and Knight. Speculation is an inextinguishable part of the discovery process.

Methodological individualism and attention for so-called microfoundations are hallmarks of the great majority of approaches in economics, including the Austrian School with Mises at the forefront (Hands, 2001). But Mises was aware that entrepreneurship is a property of individuals, not a typology or class of individuals. There is no use or need for a fully reductionist account of what goes on in financial markets.

## 4.8 CONCLUSION

Austrian market process theory can bridge some of the schism in finance between neoclassical and behavioral finance. It shouldn't come as a surprise that the Austrian School has interesting things to say about finance and financial markets. On the neoclassical side, Hayek, whose influence goes far beyond Austrian thinking (or economics) alone, spent a substantial part of his academic life in Chicago. Fritz Machlup, himself a student of Mises, was the teacher at Johns Hopkins of Merton Miller who has had a profound influence on later scholars of neoclassical finance. Mehrling (2005), while dismissing it in the end, entertains the idea that Fischer Black somehow belonged to the Austrian School. Black's view that financial markets may not be fully economically efficient, but that they are to a large extent financially efficient, is shared by many today, amongst whom Fama and Malkiel. The

Austrian and Chicago School share a preference for, and belief in, markets as coordinating institutions[93].

With the more Keynesian oriented programs Austrians share, among other things, the acknowledgement of uncertainty; in the case of the post-Keynesians even very explicitly. What they do not share, however, is the Keynesian scepticism on the working of markets. Keynes, despite being an avid investor and speculator himself, considered the stock market a beauty contest: "a game of Snap, of Old Maid, of Musical Chairs" (quoted in Bernstein, 1992). But that scepticism doesn't seem fully warranted, it appears. "The dark forces of time and uncertainty" are not so sinister through Austrian eyes. With behavioral school the Austrians share recognition of error and the faltering nature of human behavior, despite differences about how these affect aggregated outcomes.

Some Austrians may have issues with the foregoing analysis. They may object to the use of quantitative and statistical methods by Benink et al. And, as pointed out by Colin-Jaeger & Delcey (2019), despite the epistemological similarities between the ideas of Fama and Hayek, their methods were profoundly different: the analytical quantitative rigor of neoclassical finance versus the descriptive causal process approach of the Austrians. They may also object to the real world examples, used as illustrations here. But the empirical material does fit in with Hayek's more permissive methodological stance of discovering patterns.

Austrian economics can provide a bridge between the behavioral and neoclassical approaches to finance. What is more, it also provides a descriptive bridge between practice and theory, between academic finance and the real events in financial markets. That is important because it would seem that the various roles of theory and models are frequently conflated[94]. One can often encounter the idea that finance, following Milton Friedman's adage, is about meaningful predictions. At the same time, the Efficient Market Hypothesis can be interpreted as stating that there is no meaningful prediction possible. Finance theory, despite that most academic work has been and is empirical (see chapter two), is essentially a normative, prescriptive theory; not how it actually is but how it ought to be. The practical value of a descriptive account, such as proposed here, resides in identification of deeper causal processes, finding potentially relevant tendencies. Austrian market process theory can deliver those for financial markets.

The dangers of indiscriminately applying normative theory in the practice have become painfully obvious in the notorious demise of LTCM, a hedge fund that pursued an extreme

93 Quite a few prominent Chicago School economists appear to have been labeled "Hayekian" by certain authors (Colin-Jaeger & Delcey, 2019).

94 See Mäki (2001, 2006) for a broad outline of the various properties and purposes of models and theory.

arbitrage strategy (see Lowenstein, 2000; Mehrling, 2005; MacKenzie, 2006). Nobel Prize winners Scholes and Merton were actively involved in LTCM, somehow believing that the world had started resembling their model world more and more, implicitly assuming that uncertainty had become negligible or that it was transformed correctly into quantifiable risk.

It is precisely that uncertainty that has become one of the main concerns of regulators and policy makers with regard to financial markets, and rightfully so after the events of 2007-2009. Today's financial system is a global sphere with uncountable links and connections. Capital is, together with information, the fastest moving resource in modern society. It is practically impossible to attach meaningful, objective probabilities on potential dangers. But that doesn't mean that we should forget about those dangers: a system is as strong as its weakest link.

Even when the weakest link seems to give way the Austrian account of the market process provides explanation –and perhaps consolation. "Crashes" seem a lot less formidable when one regards the market as a dynamic process. It is not simply that "what goes up, must come down". Rather financial markets are a dynamic environment where, through trial and error, participants learn and discover while at the same time being put on the wrong foot again by a new sequence of events. In a way financial markets, despite all the turmoil, have proven to be quite robust and resilient institutions of society, even after the events of 2007 and 2008; the Austrian account provides a possible explanation for that.

# Chapter 5

## The Cash Value of Performativity in Finance[95]

*Performativity is a concept that relates to the idea that theory and practice interact and influence each other, a theme which runs throughout this dissertation. Performativity can be described as the idea that a theory or an aspect of a theory such as a model in some form enacts the reality it is intended to describe. This chapter scrutinizes the arguments brought forward on the performativity of finance theory, in particular as presented by Donald MacKenzie.*

95 This chapter is an extension of and builds on Daemen, 2008: Review of Donald MacKenzie An Engine, Not a Camera: How Financial Models Shape Markets, MIT Press. Erasmus Journal of Philosophy and Economics.

## 5.1 INTRODUCTION

Since the beginning of the millennium there has been renewed strong interest in finance from sociological circles. In Knorr-Cetina and Preda (2005) one can find a collection of contributions and Preda (2007) provides a systematic categorization of various strands of work in what has been labelled "the social studies of finance"[97]. That is to be commended since finance has emerged and developed as the pluralist product of a variety of disciplines (see chapter 1). It would do well, even having grown into a distinct field of its own, to keep an eye out for what is said about its subject matter outside of its own narrow scientific community.

Out of the various sociological approaches to finance, quite a bit of attention was caught by work on so-called performativity, originating in the more general performative turn in economics. Performativity of economic theory –a concept introduced by Michel Callon—has drawn considerable attention in various academic conversations (see MacKenzie, Muniesa and Siu, 2007, for a collection of thoughts on the topic). It implies that "(e)conomics performs, shapes and formats the economy rather than observing how it functions" (Callon, 1998).

Donald MacKenzie took Callon's idea to heart and attempted to apply it in an empirical way to the case of finance and financial markets. In collaboration with others, he has produced a series of papers (MacKenzie, 2003a, 2003b, 2004a, 2004b, 2005, 2006b, 2007; MacKenzie and Millo 2003, MacKenzie, Buenza and Hardie 2006) culminating in his 2006 book "An Engine, Not A Camera: How Financial Models Shape Markets". His work has received quite a bit of attention from various corners (see for instance Merton and Bodie (2004), Guala (2007), Mirowski and Nik-Khah (2007), Callon (2007), Mäki (2013), Bernstein (2007), Daemen (2008), Teira (2009)).

That attention was well deserved: "An Engine, Not A Camera" was a wonderful book: simply a good read for anyone with an interest in finance and financial markets. But it also provides a different perspective on finance: a sociological perspective, using the concepts, tools and methods of sociology, whilst not shying away from treating the dominating concepts of the discipline of finance itself.

His analysis is also of philosophical interest as an example of actually looking at "science in action" highlighting the Kuhnian notion of science as socially shaped and influenced. But the most waves have been made by the performativity claim: theory shaping and altering its

97 See also De Goede, 2005.

subject matter: the practice it is intending to describe. Judging by the received attention, it would seem that MacKenzie c.s. in examining the development of option pricing theory in combination with the rise of option markets had produced a veritable Kuhnian exemplar for performativity in economics. Indeed, performativity with regard to finance and financial markets, in particular option theory and the Black-Scholes model, is still considered as one of the landmark cases of the program.

While the initial hype surrounding performativity appears to have disappeared somewhat[98], a couple of more recent publications (Muniesa, 2014, Boldyrev & Svetlova, 2016) have shown that there is still interest in the concept[99]. Muniesa's contribution being more conceptual, for instance dealing with the discussions that have emerged about what performativity in economics exactly means, while Boldyrev & Svetlova provide a collection of essays which essentially is an update on the status of the performativity program and how it could be used. Both Muniesa and Boldyrev & Svetlova explicitly point out the work of MacKenzie, in particular the 2006 and 2007 books, as seminal with regard to performativity in economics.

It should be obvious from the earlier chapters that I share MacKenzie's attention for the important, interactive role that finance theory has played in the evolving practice of the financial markets. He argues persuasively that the theory has had a deep and profound impact on the practice, a performative impact, by which he means that the theory has helped shape the reality it refers too. However it is worthwhile to scrutinize his claims more closely, both from the angle of finance theory and from a philosophical angle with regard to his arguments for performativity of finance theories.

MacKenzie's most remarkable, and probably most controversial, claim was that the Black-Scholes-Merton option pricing model may have been performative in a very strong, so-called Barnesian, sense. Barnesian performativity entails that practical use of a model or theory shapes the process to which the theory/model refers along the lines of the theory/model. While being careful in his assertion, Barnesian performativity was where, in his own words, his main interest lied. In the 2006 book MacKenzie already admits that it might be hard to make a compelling case for Barnesian performativity. In a recent contribution (Bamford & MacKenzie, 2018) his stance appears to be modified somewhat: the phrase "Barnesian" performativity does not feature at all in the 2018 paper. Instead the focus has shifted to the opposite of Barnesian performativity: counterperformativity[100]. Counterperformativity

98 Brisset (2017a) remarks that "The two main architects of a performativist sociology, Michel Callon and Donald MacKenzie, seem to have moved on to other subjects, while the critical power attached to performativity is now firmly rejectedby several heterodox economists precisely because of its lack of a critical spirit with respect to economic theories".

99 See for instance Braun (2017), Breljak & Kersting (2017), Brisset (2017a), Kuchař (2017), Nørholm (2018).

100 In addition, the (counter)performativity claims appear to be more nuanced and a bit less far-reaching.

entails that the practice develops contrary to what the theory or model posits. In both cases theory operates as a constitutive mechanism: it creates practice.

I urge caution on making the strong, Barnesian en counterperformativity, claims, especially in the way they were presented in the, still influential, 2006 book. In what follows I will provide two objections. The first one is based on the epistemic content of option pricing theory. Second, some methodological clarification and precision is in order about when talking about theories, models, and formulas and their truth in relation to the nature of economic phenomena.

## 5.2 CALLON'S PERFORMATIVITY

The concept of performativity as such is not novel. It stems from speech-act theory and was conceived in the 1950s by the philosopher of language J.L. Austin. In brief, a "performative utterance" implies not just saying something, but also performing an action, a so-called illocutionary act. Classic examples are the naming of a ship and the phrases used in wedding ceremonies. By using certain words in a particular context something is accomplished: the ship becomes S.S. Queen Elizabeth; Tom and Jen become husband and wife.

Performativity applied to economics is of much more recent date. Michel Callon introduced it in 1998. As far as I understand it, economic theory being performative implies that it shapes and alters its subject matter: the practice it is intending to describe. Economics is embedded in the economy; they constitute a hybrid forum. That provides a reason why economics is relevant. On the other hand it also implies that it makes no sense to regard economics as a positivist discipline separate from the constructed reality of the economy (Callon, 1998). Which indicates that performativity has affinity with what has been called social constructivism or constructionism (see Hacking, 1999).

What about performativity in economics though: what does it mean for an economic theory, or a theory in general, to be performative? Callon himself sets out to answer that question in "Do Economists Make Markets? On the Performativity of Economics" (2007), edited by MacKenzie, Muniesa and Siu. Callon's starting point is clearly connected to his work in Actor Network Theory (ANT) where one of the core ideas is that of the opposing epistemological views of, on the one hand, science as a mirror of reality, versus, on the other hand, science as an intervening and transforming participant in reality (p. 314)[101]. For economics that translates in seeing economists either as describers and analyzers who produce concepts,

101 See for instance Callon (1986) and Latour (1999).

theories and tools but stand outside the real economy, or seeing them as inventors and innovators who are immersed in the economy (p. 313).

Callon clearly subscribes to the latter view, while acknowledging that the separation between representation and intervention is difficult and ambiguous. He claims that performativity can deal with both. It can do so because in the performativity view the question of truth and non-truth is displaced by the matter of success and failure (p. 320). What is observed in reality is not so much an irrevocable essence, but that what has been actualized as the outcome of a process that is highly contextual in time and place. History and setting matter, results are temporary, fluctuating and constantly under pressure to change (p. 335).

While Callon's ideas have drawn various criticisms (see for instance Miller, 2002; Fine, 2003), it is not my immediate concern here to address the problematic issues of the performativity thesis in general. My interest lies in the practical applications of the concept with regard to finance and financial markets. Callon himself does not really operationalize the performativity concept in an empirical way, but others have made the attempts to do so. In the 2007 book two efforts stand out: the work done by various researchers on the use of game theory in the FCC auctions, and Donald MacKenzie's work on finance theory in relation to financial markets. The former has primarily drawn attention from the directly involved scientific communities such as (economic) sociology, science studies and philosophy of science (see Guala, 2001, 2007; Nik-Khah, 2006; Mirowski & Nik-Khah, 2007).

MacKenzie's work has managed to attract attention outside of the immediate environment of the performativity conversation as well (see for instance Bernstein, 2007). That could be due to his reputation: MacKenzie has long been recognized as an eminent historian and sociologist of scientific knowledge dating back to his earlier work on statistics and technology. Furthermore, MacKenzie explores a whole body of knowledge containing a number of theories instead of a singular isolated case such as the FCC auctions[102]. He knows what he is talking about and he knows how to talk about it. That may explain why his arguments are regarded as persuasive enough to serve as a reference example for performativity (see Guala, 2007, Brisset 2017)[103].

102 Similar singular isolated case can be found in MacKenzie, Muniesa and Siu (2007), such as the case of the strawberry auctions (Garcia-Parpet, 2007) and the fishery quotas (Holm, 2007).

103 Brisset (2017b) labels MacKenzie & Millo, 2003, and MacKenzie, 2006b, "The two most frequently quoted articles in the literature on performativity"; MacKenzie (2006) is among the most innovative and influential in this field" (Fontaine, 2016, quoted in Brisset, 2017a).

## 5.3 MACKENZIE'S PERFORMATIVITY

In taking financial economics and financial markets as his case in point to demonstrate the entanglement of theory and practice, MacKenzie chose well. Finance theory and the practice of financial markets display a remarkable proximity in that many theoretical concepts are, in some form, used in practice. The insight that academic theory has deeply influenced the practice of financial markets is not new by any means. Peter Bernstein already stressed this aspect of finance in his classic 1992 historical account of finance and financial markets. It is also reflected in this quote from Stephen Ross (2005) on option pricing theory: "Judged by its ability to explain the empirical data, option pricing theory is the most successful theory not only in finance but in all of economics". Finance theory is part of the practice of financial markets.

A traditional view on science is that theory describes, and refers to, reality, much like traffic in a one-way direction. But the suggestion here is that we find two-way traffic, running in both directions between theory and practice. "The market provided financial economics with their subject matter, with data against which to test their models and with some of at least the more elementary concepts. Financial economics did more than analyze markets, it altered them" (MacKenzie, 2006a, p. 12). This leads MacKenzie to his claim that important parts of theory in finance have a performative trait. That would present an important challenge to traditional ideas about the relation of theory and practice.

Callon's definition is the starting point for MacKenzie: economic theory being performative implies that it shapes and alters its subject matter: the practice it is intending to describe. But then he chooses his own course. MacKenzie distinguishes three different types or degrees of performativity in "An Engine, Not A Camera". First there is the generic form which implies that an aspect (model, theory, data) of economics is used in an economic process: theory is used as a tool or instrument. Second, effective performativity involves practical, difference-making use of such an aspect: theory acts as an engine of change. The strongest variety he distinguishes is Barnesian performativity: practical use shapes the process along the lines of the theory/model. Finally, there also is the possibility of counterperformativity which is the opposite of the Barnesian kind: the practice develops contrary to what the theory or model posits. In the latter two, theory operates as a constitutive mechanism: it creates practice.

According to MacKenzie finance theory has become incorporated in the infrastructure of the financial markets in three ways (2006, pp.250-251):

- Technically, as evidenced by the use of models in trading software. For example, the use of option pricing models is ubiquitous in derivatives markets: in trading and investing itself, but also in regulation, market organization and risk management;

- Linguistically, as seen in the use of originally theoretical terminology as "beta" and "volatility" becoming standard;
- Legitimatizing: (financial) economists actively helped, for example, in the advent of derivatives markets, when options and the like were still very much seen as gambling devices[104].

It would seem that the first two qualifications of performativity clearly apply here. Finance models and vocabulary weren't only used as tools, they also changed the entire setting of financial markets. And this change of setting wasn't restricted to the narrow environment of the market itself, it stretched toward the institutionalization of financial markets in modern society. The incorporation of finance theory in the financial markets was consequential or effectively performative. Generic and effective performativity might thus not be really controversial concepts. As mentioned above, the entwining of finance theory and the practice of financial markets has been noticed by others. MacKenzie provides another excellent illustration of this.

## 5.4 BARNESIAN PERFORMATIVITY AND COUNTERPERFORMATIVITY

While MacKenzie's analysis of generic and effective performativity is interesting in itself, his self-professed main interest lied with the investigation of performativity in the strongest sense (MacKenzie, 2006 pp. 18-21). He dubbed this type "Barnesian" referring to sociologist of science Barry Barnes. MacKenzie's interest in Barnesian performativity is not surprising given his affiliation with the sociology of scientific knowledge (SSK) and the so-called Edinburgh Strong Program in particular. The concept of performativity can be regarded as an expression of the reflexivity thesis which is one of the characteristics of this line of thinking (Hands, 2001). SSK and the Edinburgh Strong Program, spearheaded by Barry Barnes and David Bloor (see for example Barnes, Bloor and Henry, 1996), are part of what is called the social constructivist approach to science (see Hands, 2001)[105]. In very general terms the social constructivist agenda posits that facts and events are not fixed and inevitable, that they are a product of history, social forces and ideology (Hacking, 1999 p. 2). Contingency and contextuality are emphasized. For science that translates into the idea that, contrary to the traditional view of science as independently describing and analyzing the phenomena in the

104 Another example would be the Efficient Market Hypothesis which was crucial in establishing the stock market as a credible and stable institution of modern society. Half a century ago stock markets were seen as an unfair playing field full of insider trading and speculation.

105 The before mentioned Actor Network Theory (ANT), spearheaded by Bruno Latour and where Michel Callon has been active in, is another prominent branch of this school of thought.

world out there, science and scientists are very much part of that world: that science and scientists intervene and partly construct that world.

Social constructivism has led to intense debates in the philosophy of science. Constructivist arguments comes in various guises and degrees (see Hacking, 1999 p. 20) not all of which are controversial but the idea of Barnesian performativity is precisely the type of claim that Hacking would call "unmasking": the extra-theoretical function takes centre stage, undermining and partially stripping an idea of its authority. The traditional view of science is put upside down. Instead of an independent reality which is described by science and scientists, we encounter the idea that it is science and the community of scientists which enact and create reality.

Let's take a closer look at MacKenzie's case for Barnesian performativity. In his journey through finance theory and the financial markets, he considers the performative aspects of the main strands of finance theory (see also chapter two): the Modigliani-Miller propositions, the Capital Asset Pricing Model and the Efficient Market Hypothesis. But the best case, in his opinion (2006a, p.256), is made by Option Pricing Theory, for the purpose of his analysis labelled the Black-Scholes-Merton (BSM) model[106].

MacKenzie's argument starts with two observations. First of all, option prices became more like those postulated by the Black-Scholes model[107]: "The fit between the Black-Scholes-Merton model and empirical patterns of option prices was originally only approximate, but it improved rapidly after the model was published and adopted by market practitioners". The value of this observation is limited though, as MacKenzie himself remarks, because the Chicago options market started in the same year as the publication of the Black-Scholes and Merton papers. But what also happened was that, as the derivatives markets started to flourish, the market conditions started resembling those assumed in the model more and more. Those assumptions were a well-functioning market, i.e. including the possibility of unlimited short selling and the absence of transaction costs. Indeed, what happened was that liquidity got better and better, and trading costs became lower and lower. The originally rather stringent assumptions became less unrealistic as the derivatives business thrived.

106 Strictly speaking this is not correct terminology. Option Pricing Theory is built on the two seminal but separate contributions of, on the one hand, Fischer Black and Myron Scholes (1973) and on the other hand Robert C. Merton (1973). Although "Black-Scholes-Merton" has become somewhat commonplace terminology, Black & Scholes on the one hand, Merton on the other hand, had quite distinct approaches towards tackling the problem of option valuation. This is also acknowledged in Bamford & MacKenzie (2018).

107 It was the Black-Scholes formulation that actually reached the marketplace by means of Black's "sheets" with option prices, which were used on the trading floor (see also the previous footnote).

So far one could deem this process simply as that of a significant discovery, a genuine breakthrough as MacKenzie himself noted (2006a, p.258), providing a crucial impetus for the derivatives industry which subsequently gathers steam . But he makes a crucial step that leads him to consider this a case of Barnesian performativity. After the 1987 crash, the empirical fit of option prices with the Black-Scholes model deteriorated.

What happened was that the prices of various options on the same underlying asset began to deviate, relative to each other. Each option price contains an implied volatility (i.e. the expected standard deviation of the underlying asset). In the original Black-Scholes model the implied volatility was assumed constant for the various options on the same underlying asset. The price of volatility started to vary with the strike price; options with lower strike prices, which become valuable when the asset price goes down, demanded a relatively higher price than options with higher strike prices. This phenomenon has become known as the volatility skew and is at odds with the original Black-Scholes model which assumes constant volatility.

So now the world of the option markets started resembling the model less, a counterperformative move in MacKenzie's terminology. The emergence of skew tells us one of two things, according to MacKenzie: "if Black-Scholes is the "right" way to price options, then the market has been wrong since 1987; on the other hand, if a pronounced volatility skew in options is "correct", then the market was wrong before 1987". The latter is more plausible, as a case of rational learning, and that makes the Black-Scholes model not a "true" "discovery of what was already there" (2006a, pp. 258-259). What happened in the reality of option pricing, in other words, was a historically contingent process in which the model played a constitutive role.

At some spots in the 2006 book MacKenzie is rather cautious in assigning the label of Barnesian performativity (see for instance p.21). Regarding the BSM model, however, in the end he is quite adamant: "the model was a theoretical innovation, not simply an empirical observation; the relation to the market was not always passive but sometimes active, not always descriptive but sometimes performative, even in the Barnesian sense" (p.259).

In Bamford & MacKenzie (2018) there is no more explicit mentioning of Barnesian performativity, but rather the focus lies on the counterperformative move. The "Barnesian" move is still there though:

Despite the many necessary caveats, we think that it is justifiable to talk of the use of the Black–Scholes model having effects on markets, among which were processes that changed the world in ways that, to put it very crudely, made the world 'more like' the model. A view of performativity, however, that focused only on these processes would be a dangerous

one, and our argument in this article is that it is necessary to give at least equal weight to processes that have the opposite effect, that change the world to make it less like the model's postulates—in other words, counterperformative processes (ibid.).

## 5.5 CONTRA BARNESIAN PERFORMATIVITY

Summarizing MacKenzie, we have a theory which is used by practioners. Empirical prices started resembling those postulated by the models derived from that theory. But then, after a major event, empirical prices start deviating from the model's prices. This final –counterperformative—move is vital in the argument for Barnesian performativity; the model is not as correct as it was previously thought. Now, in 2018 (Bamford & MacKenzie, 2018) the argument for the counterperformative move is still there, however it is no longer used as a claim for the strongest, Barnesian kind of performativity, but rather as evidence for performativity in general. In addition in terms of appreciation of the model the qualifications are less adamant in the 2018 version.

While that may be a signal of a step back in MacKenzie's ambition, as I see it, there remain problems with his arguments. First, was the model indeed incorrect? Second, and in connection to this, what can we say about models? Does for instance the failure or inappropriateness of an application of theory falsify the entire theory? Third, what do the labels "right" and "wrong" amount to in this context, i.e. the context of finance, economics and the social sciences in general? Finally, if a counterperformative move is evidence for performativity in the strongest sense, doesn't that mean that the claim for Barnesian performativity is still on the table?

Before addressing these issues, a qualification is necessary: MacKenzie's argument is mainly based on the performativity of option pricing theory in the Chicago options markets where mainly stock and stock index options are traded. But option pricing theory is about valuing contingent claims in the broadest sense; stock and stock index options are only examples of the many, many instances of contingent claims. So we need to rephrase the first question slightly: was the Black-Scholes-Merton analysis wrong with regard to the trading of stock and index options; does it matter if the behavior of volatility of those particular assets in the practice differs from what was posited in theory?

The answer is negative because option pricing theory is not a theory which claims to predict, explain, or understand volatility. Six factors affect the price of an option: the current price of the underlying object, the exercise price of the option (i.e. the price where an option becomes intrinsically valuable), the expiration date of the option, the risk-free rate of inter-

est, dividends (if applicable) and the volatility of the underlying object (Hull, 2000). The first five are usually quite readily observable. The sixth one –volatility—quite often is not. That is why volatility is what ultimately determines the value of a contingent claim, since it is the only unobservable variable in the equation: it is the explanans, the explaining variable, not the explanandum, the variable to be explained. The core insight of option pricing theory à la Black-Scholes-Merton is the fact that the value of a contingent claim depends on the variability of the underlying object, i.e. its volatility.

What can we say about volatility? There is an enormous literature on the subject. It is a measure of movement, defined as the standard deviation of returns over a certain period, actual returns that is. As such, actual volatility can only be determined ex-post; it is impossible to observe directly. That means that the unambiguously correct price of a derivative at some point in the past can only be ascertained after the contract has expired. Volatility is the only variable in option pricing theory which is unobservable, which makes its estimation the core problem of derivatives pricing, i.e. the application of option pricing theory.

Any meaningful discussion about volatility is about estimated volatility, not actual volatility. For that is what is entered when calculating the price of a current option in the market. What can be inferred from empirical option prices is the market consensus about those estimates. If one adheres to some form of randomness in the returns on assets, it should become evident how difficult it is to make those estimates or model them.

Fischer Black flat-out refused to postulate or accept any model of volatility: he considered this simply impossible (Mehrling, 2005). And this opinion is widely shared and acknowledged by both academics and practitioners. We can find it in John Hull's classic academic text book "Options, Futures, and Other Derivative Securities" (2000) which is used throughout academia: "one assumption that is clearly not true, is the assumption that the volatility is constant" (p. 446). But we also find it in a book like Sheldon Natenberg's "Option Volatility & Pricing" (1994) with which thousands of derivatives traders, amongst whom myself, have been trained.

Studies have shown that in the long run historical volatility seems to be mean-reverting and displays tendencies of serial correlation, but it is certainly not constant over time as is the assumption in the Black-Scholes-Merton analysis. The biggest problem lies in an enigma that MacKenzie is treating extensively: the shape of the distribution of returns. While in mainstream finance that distribution is assumed to be a normal, Gaussian bell curve, there is ample evidence, that this is not an accurate description (see for example Bernstein, 1992, and more extensively Lo & MacKinlay, 1999). Empirical data suggests that so-called fat-tailed distributions are a more appropriate description of the behavior of stock prices and

indices. However fat-tailed distributions can raise particular modelling problems because they can display infinite variance[108]. The (log)normal assumption is made for reasons of simplicity and tractability.

The skew phenomenon, i.e. varying implied volatilities being the market estimate of volatility in various options, is in part a reflection of the actual distribution being different from the distribution assumed in the model. For example, when a market crashes, it tends to start moving more violently; this is reflected in relatively higher prices for options which become valuable when a market falls. Another factor is that demand for such "insurance-type" options by institutional investors is typically higher, especially when expectations of significant events play a role, for example global turmoil or the millennium change. Moreover, in times of turbulence and increased uncertainty liquidity tends to deteriorate, resulting in higher option premiums. Sellers of option, who basically take over the risk from buyers, will demand a higher price; a simple matter of supply and demand[109]. There exists substantial evidence that not all volatility arises from moves in the underlying fundamentals or expectations about such moves. Trading activity itself appears to affect volatility, further complicating a meaningful analysis of it.

Finally, why then that the skew phenomenon only emerged after the 1987 crash? MacKenzie partially provides the answer when he talks about the empirical success of the Black-Scholes-Merton model being a historically contingent process. I agree. It took an event of considerable proportions to bring volatility skew on stage. The market simply wasn't aware before; indeed a process of rational learning as MacKenzie suggests. The preeminent problem of trading options is what volatility to plug into whatever pricing model is used. Any observable option price is thus a product of market consensus (built on the estimates of the various market participants), not from an objective form of statistical inference[110]. People's perceptions change by such momentous events. If a market has crashed, it is likely that their perceived uncertainty, their risk aversion and sense of urgency rise, even in "normal" times. This can

108 The possible accuracy of infinite variance distributions, as put forward by Benoit Mandelbrot, raised widespread concerns amongst economists over the validity of their results, which were based on normal distributions. The issue was settled in a rather pragmatic way by deciding that the assumption of normality was the best workable hypothesis (Mehrling, 2005; MacKenzie, 2006a; Bamford & MacKenzie 2018). The abolition of wild randomness, infinite variance models and the like in favor of the random-walk assumption/lognormal distribution is one of the great and often forgotten stories in economics/econometrics.

109 While skew is usually most visible in out-of-the-money put options which offer protection against a downturn in the market, one can occasionally also witness it in out-of-the-money call options which become more valuable when prices rise sharply. By the end of 1999 in the heyday of the dotcom era certain stocks shot up so violently day after day that each day all call options ended up being in-the-money. New out-of-the-money options would be introduced each day. Driven by huge demand and limited hedging possibilities by spreading, these options were relatively more expensive than in-the money options.

110 Although market participants may, and often do, use historical data to estimate volatility, in the belief—or hope- that history reflects some form of fundamentals.

result in a higher price for insurance, hence a higher implied volatility in option prices[111]. So what did change after the 1987 crash was the method of estimation. It only makes sense that people try to anticipate such events in a way, hence the volatility skew. If they were right to do so can only be determined afterwards; that is the nature of volatility.

The previous paragraphs aim to show that volatility is by no means a straightforward concept. The ambiguity of the distribution of returns is due to an old economic concept, most prominently treated in the 1920s by John Maynard Keynes and Frank Knight: radical uncertainty. As mentioned in chapter four Knight (1921) makes a threefold distinction. First, there exist a priori probabilities: absolute objective chances like those in throwing a fair die. Second, there are statistical probabilities: objective, empirical evaluations of frequency of association. And third, there are estimates: subjective, more or less educated guesses, liable to error. The first two fall under the heading of risk. When we speak of risk, the distribution is known, either a priori or through empirical work, and we can obtain objective, measurable numbers. The third category is that of true radical, non-measurable uncertainty. According to Knight, it is this third category which characterizes the economic domain, or "business" as he calls it, in reality. It is only logical then that financial assets, which are basically claims on "business", display the same characteristic[112].

The consequence is that the familiar mean-variance approach to asset returns can be seriously unrealistic. The economist's way to deal with this problem is making simplifying assumptions. In the case of the original Black-Scholes-Merton analysis an assumption was made regarding volatility: that it was constant. This assumption followed logically from the log-normal distribution assumed in other dominant models in finance, in particular the Efficient Market Hypothesis and the Capital Asset Pricing Model. The assumption was unrealistic and turned out to be even more unrealistic than perhaps initially thought. But unrealistic assumptions are part and parcel of economic theorizing. As Harrison (1997) notes: "Thus we have an interesting (ongoing) tension between reality and the abstract theoretical ideal. In economics, this tension is not unique to finance, and it may well be an inevitable by-product of modelling".

The scholars involved in the development of Option Pricing Theory seem to have been aware of this (see also Brisset, 2017b). One can find it for instance in Merton (1975) where he writes about the shortcomings of finance and the "ivory tower nature" of the assumptions. And in 1988 Black published a paper with the telling title "The Holes in Black-Scholes".

111 This is only a brief, simplistic account of volatility skew. The point is that skew is not a complete mystery. See Brisset, 2017b, for a more detailed treatment of the skew phenomenon and the intricacies of the distribution of returns, which results in a similar conclusion.

112 2006 Nobel laureate Edmund Phelps made the same observation in a op-ed piece in the Wall Street Journal in relation to the 2007-2008 crisis.

But even in the two seminal contributions, Black & Scholes (1973) and Merton (1973), one can already find awareness of the problematic nature of the constant volatility assumption. In Black and Scholes for example: "The valuation formula assumes that the variance rate of the return on the optioned asset is constant. But the variance of return on an option is certainly not constant: it depends on the price of the stock and the maturity of the option" (1973, p.652). Likewise, Merton writes in a footnote that "the expected return is not directly observable" (1973, p. 161) and later remarks that if investors do not agree on the particular variance, they will arrive at different values for the derivative security (1973, p.169).

Recapping the argument: constant, linear volatility is an assumption, not a hypothesis or truth claim in itself. The Black-Scholes-Merton analysis was aimed at explaining option pricing, not the behavior of volatility.[113].

## 5.6 THEORIES, MODELS AND FORMULAS

If a particular model fails in the wake of unexpected events like the 1987 crash or the 2007-2008 crisis, does that discredit the epistemic value of the entire theory to which that model belongs? Cartwright (1999) sees the difference between a theory and a model as residing in the degree of articulation: theory is a large-scale, not necessarily formalized outline, whereas a model gives a more specified formalized depiction. In this case, option pricing theory is the body of knowledge concerned with the valuation of contingent claims in the broadest sense. This includes the familiar options, traded on exchanges or Over-the-counter, on various securities but also for example the valuation of stocks and bonds as contingent claims on a firm, and the valuation of investment opportunities which have a fixed initial price but whose potential payoff is uncertain, the so-called real options. Models then are used to construct a representation of a specific category of contingent claims: for instance the Black-Scholes model for valuation of stock options. Assumptions about the behavior of the underlying entity need to be spelled out here. Finally, a formula provides a formal recipe which can be applied to a particular contingent claim: for example the Black-Scholes pricing formula for a European call option on a non-dividend paying stock. Specific boundary conditions are entered in the model to end up with an unambiguous number: the supposed price of that option.

113 See also Brisset, 2017b, for a similar analysis of the linear volatility assumption in relation tot he supposed Barnesian performativity of the Black-Scholes-Merton model. Brisset adds that market prices of options already deviated from the prices which the BSM model would calculate even before the BSM model was actually introduced on the exchanges.

What MacKenzie shows is that a pricing formula which at first seemed to be substantiated by empirical observation, later on did not to fit so well. This failure can be traced to a specific assumption in the model. By no means is the whole theory rendered void and the model meaningless. Scientific knowledge is and never has been static. It is not at all unusual for theory to grow and develop, especially theory that is part and parcel of the reality it refers to and even more so in the social sciences. The original findings of Black, Scholes and Merton are not so much about the exact price of options, but they have delivered a mechanism of contingency, key insights on risk, uncertainty and time. Moore and Juh (2006), using data from the Johannesburg Stock Exchange in the period 1908-1922 give evidence that investors had an intuitive notion of derivative pricing, long before the emergence of BSM-style derivatives pricing. Considering this, the results of Black, Scholes and Merton can be regarded as formalization, rather than a discovery of something entirely new. Their findings have been the take-off point for a "revolution", as Merton Miller (1999a, 1999b) calls it. Miller talks about the original Black-Scholes-Merton analysis as a model-T Ford: outdated and primitive. "Financial engineers have already reduced the original Black-Scholes-Merton formula to Model-T status". Indeed I am not aware of any derivatives trader today that employs the formula in its original form.

But it has been the take-off point for a research program, in which many various applications have been initiated, and many of the unrealistic assumptions and weak points, the "holes in Black & Scholes", so to speak, have been tackled and improved upon. The old-timer has turned into a multi-purpose, all-terrain hybrid vehicle, able to deal with all kinds of contingent claims and different boundary conditions[114]. However, there would not have been such a vehicle, had there not been a Model-T Ford. Odds are that something must have been "right" about it.

Typical of economic models is that they often isolate and idealize a feature (see Mäki, 1992, 2009, 2018). This doesn't need to be a problem if the isolation has an essentialist character: the theory captures a primordial causal mechanism, the model reveals something crucial. As such unrealistic assumptions can be (and often are) part of economic theorizing. But the truth of a model doesn't necessarily reside in the assumptions being realistic, rather it resides in capturing something meaningful. Mäki (2006, 2011) calls this central aspect a truthbearer: the core assertion, given various assumptions, which states a claim to (possible) truth. I have argued that in option pricing theory as conceived by Black, Scholes and Merton this claim to fame is the –somewhat counterintuitive—insight that the price of a derivative crucially depends on the variability of the underlying, its volatility. The fact that volatility

114 See, for instance Merton (1998) and Miller (1999a) .

is assumed linear and constant in the model, which it is most often not in reality, does not make the model wrong.

While it can be argued that isolation and idealization are present in any scientific endeavour, their presence in economic theorizing is particularly relevant. That is because economic phenomena do not behave according to universal, iron laws of nature but often appear to be rather more like tendencies. In economics and the social sciences in general typically many causal factors are simultaneously at work instead of one or a few which can be singled out[115].

Dan Hausman, for instance, has argued that economics is a "separate and inexact science" (1992, 2001). In the case of economics, the multiplicity of causes and their interaction gives rise to what he calls inexact laws. This applies to finance as well, I believe. Asset prices reflect certain "objective" fundamentals, but they also reflect subjective expectations and intuitions of a more tentative nature. Despite the ubiquitous presence of formal modelling and quantitative approaches, and boatloads of empirical data to back up claims, finance is still a social science, as is economics. Dating back to Adam Smith's invisible hand, at the heart of the matter are the unintended consequences of human action and their interactions. Often the outcome of this process is uncertain. This is illustrated in the financial markets where there are no fixed recipes for success: even the best and brightest occasionally take their lumps[116].

Making assertions about right and wrong in such a setting is a perilous affair. Yet that is what MacKenzie did in 2006 in making his argument for Barnesian performativity: "if Black-Scholes is the "right" way to price options, then the market has been wrong since 1987; on the other hand, if a pronounced volatility skew in options is "correct", then the market was wrong before 1987". That was somewhat surprising for someone with strong affiliation to the Sociology of Scientific Knowledge (SSK) program which devotes much attention on the complicated and reflexive relation between science and reality. MacKenzie 2018 appears to be more nuanced, though the counterperformativty arguments still appear to hinge on some form of truth appreciation.

That the BSM-model is not simply wrong was argued extensively above (see also Brisset, 2017b). As far as it concerns the market being right or wrong it might be fitting to paraphrase a quote from John Maynard Keynes: "markets can remain wrong longer than you can remain solvent"[117]. It only makes sense that, if "business", i.e. economic activity, is characterized by

115 This idea traces back to the work of John Stuart Mill (1844) which has had significant impact on many who are active in the field of methodology and philosophy of economics.

116 The case of Long Term Capital Management, mentioned in chapter three and also treated by MacKenzie (2003a, 2006a), remains an excellent illustration of this. See also Lowenstein (2000), Scholes (2000), Stulz (2000), Mehrling (2005).

117 The original quote is that "markets can remain irrational longer than you can remain solvent".

uncertainty like Knight argued (see chapter four), financial markets where claims on these businesses are traded, are also characterized by that same uncertainty. The upshot is that we can only truly assess "right" and "wrong" in the market a posteriori.

## 5.7 PERFORMATIVITY: PRAGMATICS AND PERSUASION

Where does that leave performativity in finance; is it a straw man? I think not: performativity is an interesting and useful concept as long as we stay away from the strongest varieties. Those strong claims are ill-advised because attributing right and wrong is ill-suited for the domain of finance where theory and practice intermingle. Bruno Latour has stressed the pointlessness of all kinds of bipolar distinctions such as theory and practice: "The difference between theory and practice is no more a given than the difference between content and context…it is a made divide, unity that has been fractured" (1999). The classic view of science describing and referring to an objective reality, "the world out there" so to speak, has been found lacking. But the strong, Barnesian performativity claim does exactly the same, only in the opposite direction: science creating a beforehand non-existing reality[118]. The relation between science and the world is not one-way traffic in either direction but rather two-way traffic.

What performativity in its less extreme versions does, is precisely recognize that two-way traffic. It provides us with a viewpoint to deal with the blurred complex reality, in which finance theory and the practice of the financial markets both are located. More specifically, it points at two important aspects of theory: what we can do with it and how that is accomplished.

What we can do with theory is a matter of pragmatic value. That value has been extremely high. There is no theory in finance which has had a bigger practical impact than option pricing theory: "A few academic scribblers have created a huge and still-growing industry" (Miller, 1999b). The trillion dollar derivatives industry hardly existed before Black-Scholes-Merton. But the products did already exist: options, contingent claims have existed for centuries in formal and less formal appearances. And agents using them had some intuitive grasp on their valuation (see Moore and Juh, 2006). If we want to use the performativity vocabulary, then we should talk about effective performativity here. Moreover, such a

118 In additon, Mäki (2013) adds that the expansive use of performativity blurs the difference between constitutive and causal relationships, both of which actually are involved in economics and the economy. MacKenzie clarified his position in 2018: "We are dealing here not with matters of the philosophy of language, not with 'acts [that] are constituted' by utterances, but with causal effects" (Bamford & MacKenzie, 2018).

pragmatic reading would seem to be in accordance with Callon's idea of truth and non-truth being displaced by success and failure.

How that practical success is accomplished is above all a matter of persuasion. A theory or model as such, doesn't do anything by itself; it is people who do something with it. And while they may be involved in some capacity, it isn't always the scientists who do something with it. The involvement of scientists can range from mechanism design, as in the FCC auction, to lobbying, as in the case of the Chicago derivatives markets, to active participation. The latter ranges from the various professional money management activities of finance scholars (see chapter three) to Fischer Black selling sheets with option prices to market makers. But it is hardly ever the case that economists actually make markets themselves, to answer the suggestion of the book title of MacKenzie, Muniesa & Siu (2007): "Do Economists make Markets?" A more apt description would be that they are involved in spreading the news. Deirdre McCloskey (2008) has extensively argued for the role of persuasion in economics, including the role of talk, linguistics and communication in the marketplace, in economic life. Theories, models, ideas are used because a sufficient number of people are convinced of their adequacy or usefulness. As Mäki (2013) notices:

> Literally speaking, economic theories do not shape the economy. Nor does economic inquiry. People do. In their various roles (as policymakers, students, investors, entrepreneurs, workers, consumers) people are exposed to the results of economic inquiry and they learn, directly or indirectly, about the contents of economic theories, explanations and predictions, and are inspired by them, perhaps by being persuaded by the proponents, so as to modify their beliefs and perhaps their motives. These modified beliefs and motives make a difference to their behavior, and this has consequences for the economy. The flow of these complex connections is a matter of indirect causal influence rather than direct constitution.

To some extent MacKenzie appears to agree with the above when he quotes Judith Butler as saying that "financial theories . . . do not function as sovereign powers or as authoritative actors who make things happen by saying them" (Bamford & MacKenzie, 2018).

The cash value of performativity, I believe, resides in its emphasis on the pragmatic and persuasive roles that theory and models can possess. Those roles are particularly relevant in a situation where theory and practice are entwined, as is the case for finance and financial markets. But the pragmatic and persuasive functions stressed by the performativity approach, are not the only roles that are performed by theory and models; engine and camera, and some more.

## 5.8 CONCLUSION

The impact of finance theory on financial markets has been used as a premier example of what has become known as the performativity thesis. This case, as most prominently brought forward by Donald MacKenzie, has been scrutinized from two angles. First, from an epistemic viewpoint by looking at the contents of the particular theory itself. Second, from an methodological stance by reflecting on the distinctions between theories, models, and formulas, and their truth content and the nature of economic phenomena.

While MacKenzie provides an excellent illustration of performative aspects of finance theory, he does not present a convincing case for the strongest instances of performativity. The empirical observation of the emergence of volatility skew does not refute option pricing theory, as conceived by Black, Scholes and Merton. It is only proof of one assumption being false and/or unrealistic, an assumption which is and was well-known to be problematic and not to be accurate.

More in general it was argued that the bonds between reality and knowledge of that reality are complex in this case; at the least too complex to simply dish out labels of "wrong" and "right". Alternatively it was suggested to consider performativity as highlighting pragmatic and persuasive functions that models and theory can have, while at the same time acknowledging that these are not the only, or a priori most important, roles. Such a reading would also appear to be in accordance with the broader literature on performativity, in particular with Michel Callon's writings.

Perhaps the metaphor of maps can serve to illustrate. Almost all world maps we use are false and/or wrong and/or unrealistic in a great number of aspects. The world is portrayed as flat while it is not. The sizes of the continents, counties, oceans, etc. are often not accurate. Countries and terrains are usually represented in some color scheme just for the sake of distinction. And so on, and so on. Yet we all use these maps. We use them because there are also important things right about them. Ergo, they even affect our actions and enterprises, a performative effect. They capture something that apparently is of great use to us and that we are convinced of that it is of use. I suggest that likewise is the case for option pricing theory.

The performativity thesis can be regarded as an interesting way of looking at science in its workings and its effects on our daily lives. But it should not be regarded as a full-blown, universal statement about finance and economics[119].

119 MacKenzie appears to have come to the somewhat the same insight when he discusses the place of performativity and counterperformativity in the analysis of finance in the 2018 paper: "Neither concept is a panacea; both can only be supplements to other forms of enquiry, including more traditional political economy" (Bamford & MacKenzie, 2018).

# Chapter 6

## The Great Financial Crisis and a Pluralistic Way Forward in Thinking about Financial Markets

*This research project first began to take shape in 2005, i.e. before the 2007-2008 crisis broke loose. Since 2007 financial markets and the thinking about financial markets have profoundly changed. One may think that because of these changes, parts of the analysis in this dissertation have become obsolete. This is not the case. Rather, the relevance of many of the points made, is emphasized by the events that have unrolled since the ignition by the subprime crisis in 2007.*

*This chapter is by no means an exhaustive and comprehensive account of the crisis and its consequences for theory and practice of financial markets, but deals with some aspects of the crisis which relate to the preceding chapters. Subsequently suggestions are made which could improve the conversation on financial markets*[120].

120 And there are even more alternative approaches such as fractal finance, evolutionary finance, bubble theory and so forth. See Pistorius (2015).

## 6.1 INTRODUCTION

> "Of all the economic bubbles that have been pricked, few have burst more spectacularly than the reputation of economics itself"[121].

As a result of the crisis much criticism has been hurled towards the science of economics, both from inside the profession[122] as from outside. Economists haven't been able to soundly analyse fundamental developments. Their theories and the assumption used were lacking. Like the bankers themselves and their supervisors and regulators, economists have missed what prolific dangers and risks were creeping into the financial system.

The criticisms concern both academic economic knowledge as well as the use of that knowledge by policymakers and practitioners in the financial sector. Critical analysis has been done both by academics and non-academics such as journalists and policy makers. The fact that a broader audience has raised an interest in financial markets may actually be a positive to take away from the crisis, but the general perspective from the general public on finance as a business, but also as an academic discipline has undoubtedly deteriorated (Zingales, 2015).

Lo (2012) provides a useful overview of 21 books by academics, journalists and others where various aspects of the crisis are highlighted. And since then many more analyses have appeared. Examining the various narratives and the evidence presented, he concludes there is not one comprehensive account and that one should be careful and precise in analysing the stories presented.

## 6.2 VARIOUS CRITICISMS

It is useful to distinguish between various criticisms. Saying that macroeconomic models do not predict well is a very different claim from stating that pricing in financial markets is not proper. Roughly speaking, a distinction can be made between criticisms aimed at macroeconomics and critiques on financial economics. Regarding the former, it is a well-known fact that most macroeconomic models that were used for policy purposes were Dynamic Stochastic General Equilibrium (DSGE) type models. These models focus on the real economy and leave out what happens in the financial sector. Implicitly it is either assumed that financial markets work so perfectly that fall-out to the real economy is not an issue, or that events in financial markets are inherently irrelevant and have no bearing on the real economy. For instance so-called asset inflation – rising prices in asset categories, think

121 The Economist, July 18 2009, section titled "What went wrong with economics".
122 See for instance Paul Krugman's op-ed piece in The New York Times Magazine, September 2 2009.

stocks, commodities or real estate– did not enter as a factor in most policy models. Generally price stability in the real economy, of goods and services, was the core target for policy makers. As long as expansive monetary policy, which has characterized most of the time period leading up to the crisis, did not translate in inflation in the real economy, it was not considered a problem. Thus a situation with moderate economic growth and stable inflation, once known as the "Goldilocks" scenario, was deemed possible. In such a situation there is no reason or incentive for policymakers to consider a more restrictive course of action. Rising prices of financial assets and the accompanying rise of debts to finance those assets did not enter the policymaking equation. Thus speculative bubbles could develop unimpeded. This was precisely what happened in the US housing market. The flood of funds resulted in diminishing returns and rising asset prices. In the quest for profitable opportunities credit standards were loosened, in particular for mortgages. In combination with rising housing prices this ultimately resulted in the subprime crisis in 2007, which proved to be the catalyst for the great financial crisis in 2008.

Parallel to the barrier in policy making between the real economy and the financial sector, there has been a growing disjunction between economics and finance, as discussed and illustrated in chapter two. The crisis has painfully revealed the undesirability of this situation, both in the area of theorizing and where it policy making concerns, and only underscores the need for a broader, more inclusive approach to financial markets.

Financial economics has received its share of critique as well. Advances in finance have enabled certain markets to develop, a point examined in chapter five. The prolific rise of derivatives markets and markets for complex structured products has been fuelled to a large extent by theoretical progress in valuation models for such products.

These models, and/or the assumptions in those models are wrong, is the critique. Throughout the preceding chapters (in particular chapters four and five) the issue and peculiarities of models and theories being "right" and "wrong", of truth and falsehood, were extensively discussed. Concluded was that one should be careful and precise in what exactly the truth claim is. Regarding assumptions, it is accepted that assumptions are made for reasons and that false assumptions do not necessarily invalidate a model or theory. Nevertheless suspicion and criticisms persist. A prominent example is the assumption used in many pricing models that markets function properly. In other words, some form of the Efficient Market Hypothesis is assumed. In the eyes of many this assumption has been clearly proven false: how else to explain the colossal bubbles that developed? Indeed the crisis has shown that financial markets and participants in those markets can err spectacularly in their valuation assessments. It is beyond doubt that speculative bubbles have formed, for instance in the American housing market in 2006, as illustrated for instance by Shiller (2008). But that does

not necessarily imply that these markets do not function properly: it has been shown that bubbles can develop in a framework where rationality is assumed (Blanchard and Watson, 1982). Also, the degree of (ir)rationality that agents exhibit or information asymmetry does not necessarily explain the formation of bubbles, as suggested by Shiller and Akerlof (2009). In conclusion and in line with the preceding chapter: the problem is not so much that the models and theories have failed but rather but that the truth claims of these models has too easily been taken for granted. Rather, the limitations of these models, in particular some key assumptions were neglected.

In chapter four it was discussed that the market process is a tentative process where various opinions, based on discovery and learning, come together. The result of that process is a price. That price, the market price, is an estimate of future events. These future events are shrouded in uncertainty (of the Knightian kind) because economic processes remain in essence social processes which are not subject to iron laws, unlike phenomena studied by natural sciences. So markets can get it massively wrong, but it will correct itself at some point by discovery and learning, or so it is assumed. That appears to be what indeed happened in the market for structured products, consisting of securitized mortgages (CDO's, etc.) according to Gorton (2010): the market was slow in catching up with reality. The market's inability to correctly assess developments may have been due to the complex nature of the products involved, resulting in a lack of transparency to market participants, supervisory bodies and regulators. When the market finally got it in 2007 the landing would be hard and painful.

This lack of transparency is a different issue than a question which has also been raised: whether certain financial products and practices in financial markets have any usefulness and legitimacy at all. Some have suggested that the whole process of packaging mortgages into securities and subsequent slicing up and repackaging of these securities into products such as CDO's (called the originate-to-distribute model) is kind of a Ponzi scheme designed to con less sophisticated investors and arising from information asymmetry (Akerlof & Shiller, 2009). One should keep in mind, however that the securitization process (and the products resulting from it) is actually a device which helps both buyers and sellers accomplish their desired risk-return profiles. If anything, it is the pricing and risk perception of these products that went wrong. Structured products such as CDO's offered a significantly higher projected return than other bonds with similar credit ratings. However, the losses arising from these products were not only incurred by buyers but also by sellers, typically investment banks, who kept parts of these products on their own balance sheets and held a stock of loans to be repackaged later. This so-called warehousing was responsible for some of the most staggering losses at individual financial institutions which participated in that business. That would

imply a general lack of knowledge, unawareness, ignorance and transparency on both sides of the market, rather than information asymmetry that is exploited by one side[123].

This also raises the question who within the practice of the markets truly believed in market efficiency. Lo (2011) makes the interesting remark that those who adhere to market efficiency should have been less likely misled by this pricing inconsistency. Lo's point is that the Efficient Market Hypothesis does not imply that any price in the market is right. Rather, it is deemed unlikely or impossible that similar products, such as bonds with similar credit ratings, consistently display very different prices.

The key question then becomes, not if we should get rid of certain markets or products, but how to improve the market process and limit the damage of corrections. An example is the existence of so-called Over-the-counter (OTC) markets where parties deal directly with each other instead of trading on a regulated marketplace like a stock exchange with central clearing and settlement. OTC markets are less transparent and more susceptible to counterparty and concentration risks[124][125].

The danger surrounding the Efficient Market Hypothesis is not so much if it is true or false but rather its indiscriminate and careless use as an assumption. The EMH pertains to a different degree to the market for mutual funds than to a market in some OTC tailor-made exotic structured product. The former is a market which is transparent, liquid and filled with plenty of agents on both the supply and demand sides so market power is fragmented. Valuation is relatively straightforward and the pricing process is transparent. The latter is a market where information is complex, therefore pricing is not easily comprehensible. Only a few can provide such a product. Moreover, it is not a uniform product which can make it harder to trade away or hedge when conditions should warrant that. In the latter case the conditions required for an efficient market are clearly less satisfied than in the former case: more market concentration, more complexity and therefore less transparency and a lesser degree of atomistic agents.

123 One could include stupidity. In July 2007, when the liquidity crisis was already underway. former Citicorp CEO Chuck Prince told the Financial Times that global liquidity was enormous and only a significant disruptive event could create difficulty: "As long as the music is playing, you've got to get up and dance," he said. At that time he added: "We're still dancing". Those words would come back to haunt Prince: in the aftermath of the fall of Lehman Brothers, Citicorp needed two capital injections from the government totaling $45 billion and received government backing for loans and securities, worth more than $300 billion at the time.

124 The market for Credit Default Swaps (CDS) was such an OTC market. It turned out that an enormous amount of the risk in the CDS market was concentrated within one party, the US insurer AIG. This made AIG too big to fail: if AIG would not have been rescued, it would have caused potentially fatal damage to a number of other institutions which had entered into deals with AIG as counterparty and may have triggered chain reactions in other markets.

125 Many of the dangers and frailties that could be present in OTC markets have been adressed by new regulation.

In chapter 1 three basic principles for a proper market were identified. Legitimacy: what is traded on a market should be acceptable to society and have some kind of usefulness. Transparency: the process of price formation should be transparent and the information equally accessible to all participants. Liquidity: a market needs sufficient potential supply and demand interest to function properly. In the crisis the lack of transparency led to stalling liquidity in some parts of the markets in August 2007 which subsequently infected the entire financial system, culminating in the fall of Lehman Brothers in September 2008. Already in August 2007 Caballero and Krishnamurty wrote:

> Uncertainty –that is, a rise in unknown and immeasurable risk rather than the measurable risk that the financial sector specializes in managing– is at the heart of the recent liquidity crisis. The financial instruments and derivative structures underpinning the recent growth in credit markets are complex. Because of the rapid proliferation of these instruments, market participants cannot refer to a historical record to measure how these financial structures will behave during a time of stress. These two factors, complexity and lack of history, are the preconditions for rampant uncertainty.

In such a situation, the principles for a proper market are violated and the chances of market failure become higher. In the Austrian terms, discussed in chapter four: the market process of entrepreneurial discovery is more complicated and hindered in the latter case. Entrepreneurs have difficulty in choosing their actions, will become more tentative in an uncertain environment and an equilibrating move will take more time. The focus should then be on how to improve the functioning of that market because the degree of efficiency is the result of the particular market process and not a given in itself[126].

Criticism has also been directed at valuation models and risk management tools which have their origins in mainstream financial economics. For instance the Value-at-Risk (VaR) methodology that was and is widely used in risk management. VaR-models use historical data to calculate profit and loss projections for particular statistical significance intervals. If an improbable event takes place that falls outside the statistical range, the impact can be devastating if risk managers have uniquely relied on VaR analysis. Some of the happenings during the great financial crisis were clearly such so-called tail events where risk managers ignored uncertainty of the Knightian kind. The fall of Lehman Brothers is the prime example. In the past there always had been an orderly solution when an important financial institution ran into trouble, for example Bear Stearns in the spring of 2008 and hedge fund

126 "Improving the market process" is by no means unambiguous. For instance, when physical floor trading of securities is replaced by screen-based electronic trading, in general pricing improves and transaction costs go down. However, trading algorithms are typically top-secret so transparency may be deemed less. In addition, electronic trading can carry systemic risks.

LTCM in 1998. It was deemed inconceivable that an institution like Lehman would be allowed to go bankrupt.

Another example of the impossible happening was the stalling and drying-up of certain markets. For instance the repo market, a crucial marketplace for bank funding, had always been extremely liquid. It was beyond belief for the players in those markets that such a market simply stopped functioning. When it did, a number of banks ran into big troubles. The danger of a tail event is not so much the event itself but its surprise effect. If no one expects such an event or everyone deems it too improbable, no one will be prepared when it comes and the impact can be devastating. Quality stress testing can help to identify the type of low probability-high impact events that escape regular (statistical) analysis but need to be addressed anyway. If dykes are designed to contain floods in 99.99% of all cases, the impact of the 0.01% chance may justify even higher dykes.

If we follow the idea of the market as a process, outlined in chapter four, it would be recommendable if more attention would be paid on the robustness of the system: how a system copes with a shock/crisis is perhaps an even more vital question than the all-out prevention of all possible shocks and crises. For if radical uncertainty is acknowledged as part of the economic world, it becomes simply impossible to foresee all possible potential major disturbances.

In chapter two it was shown that much of current finance research consists of empirical analysis of historical data. While this type of analysis may yield valuable insights, there are dangers in focusing too much on the facts of the past. A statistical relation is not the same as a causal relation. The tag "past results are no guarantee for future performance" is usually attached to any advertisement for a financial investment. In 1998 LTCM ran into trouble when prices of various assets started deviating from their statistically solid historical relations as a result from turbulence in the markets stemming from the Asia and Russia crises. The correlations that were crucial for LTCM's arbitrage strategies changed, resulting in huge losses. Something similar played a role in the great financial crisis. Valuation and risk of complex structured products such as CDO's depend to a large extent on the correlations between the various parts that comprise such a structured product. Estimates of these correlations were based on rather limited historical data and turned out to be quite different in both number and nature[127].

Again the question can be asked if these models were wrong then? In chapter five this issue of "false" theories and models was directly dealt with in regard to option pricing theory. It

127 See Salmon, F., Recipe for Disaster: The Formula That Killed Wall Street, Wired Magazine 17-03, 2009. See also MacKenzie and Spears (2014).

is argued there that it is not the theory itself but rather an important assumption that is the cause for the anomaly. It is similar here: the value of a structured product depends on the correlations of its comprising parts. The determination and nature of that correlation is a whole different story. If particular assumptions surrounding a theory fail, the theory using those assumptions can become inappropriate though not necessarily wrong.

Perhaps the most fundamental disappointment for the general public is that economists have not been able to predict the great storm that would descend upon the world. Somehow it appears to be expected from economists to warn the public in time. The public might have been led astray by Milton Friedman's famous article (Friedman, 1953) which can be read as to say that economic theories should be judged by their ability to predict[128]. But prediction is not be equated with forecasting. Prediction is claiming that if x happens, y (and perhaps z) will happen. If we raise the money supply above production growth, inflation will pop up, perhaps later translating in unemployment. Forecasting is saying what stocks are going to perform well; a complex issue depending on many variables[129]. As evidenced by the analysis in chapter three, economists, even the very best and most highly respected, are not extraordinarily good at this. At least they do not appear to fare better than others in the investment industry who may not have an economic background or a fantastic academic resume. So in a sense the predictive powers of economists should not be overestimated.

Of course there have been economists who saw the crisis coming or who claim to have seen it coming. But that raises an additional question: why were these visionaries not heard? Apparently their stories were not persuasive enough to convince colleagues, policy makers and politicians, supervisors and regulators, bankers or the public at large. How credible would such claims be anyway? It would appear almost impossible to perfectly predict timing and scale of a highly complex phenomenon like the great financial crisis. That is not to say that economists are useless in this regard. Rather than predicting events they can point out possible outcomes and consequences of actions. They can signal important developments and potential risks. The challenge then becomes getting these signals taken seriously, in particular when they come from beyond one's own particular economic conversation or school of thought. Above all, economist can learn from the past, from the crisis, and perhaps open up a bit beyond their own niche area of expertise. For instance by paying more attention to the practice of the real world, but also to other approaches and disciplines.

The crisis has indeed ignited discussion within the discipline of economics. All too often these discussions take the shape of an ideological battle between various schools of thought,

128 How Friedman's article should be or can be interpreted, see Mäki, 2009.

129 Pistorius (2015) makes a slightly different but similar distinction between prediction and profitable prediction.

in particular so-called "saltwater" versus "freshwater" economists[130]. All the differences of opinion on the working of markets, rational behavior of agents, effectiveness of economic policy and the like are raised. In truth both camps share some of the deficiencies that have played a role. As Dow (2012) remarks, both the New Classical and the New Keynesian school treated the real economy and money-and-prices separately, thus neglecting asset inflation and leaving the formation of speculative bubbles unimpeded[131]. Instead of fighting wars of the past, a constructive future-oriented approach aimed at dealing with a globalized and financialized world might serve better. This is especially true for macroeconomics where for a long time both sides have underestimated and misjudged the importance and impact of financial markets, both in itself and in relation to the real economy. In finance some of the most ground-breaking advances have actually been combined saltwater-freshwater products: Merton Miller from Chicago and Francesco Modigliani from MIT on capital structure, Eugene Fama from Chicago and Paul Samuelson from MIT on efficient markets, William Sharpe and John Lintner on asset pricing, Myron Scholes from Chicago, Robert C. Merton from MIT and Fischer Black who worked at both institutions, on derivatives pricing.

Finance has room for some critical reflection of its own as well. Financial economist Andrew Lo (2012) points out that the crisis has touched upon basic tenets of financial economics. That doesn't so much concern particular theories but rather the general self-image of the field:

> Many of us like to think of financial economics as a science, but complex events like the financial crisis suggest that this conceit may be more wishful thinking than reality.

Lo mentions complexity and human behavior as crucial factors why analyzing a financial crisis is different from analyzing an airplane crash. The causal chain is not as clearly identifiable. Whereas the number of potential factors is limited in a plane crash (say: weather, pilot skill/human error, material defects), financial markets are thoroughly global and influenced by an abundance of factors. The point is that finance may very well be a science, but that it is in any case a social science. The outcomes are the result of complex interactions on all kinds of levels and may not always be predictable.

Chicago economist Luigi Zingales, in his capacity as president of the American Finance Association (AFA), has weighed in on finance in relation to society in his 2015 presidential address. Essentially being pro-markets but not pro-business in its current inception[132],

130 See for instance The Economist, Economists debates: Keynesian principles, March 18, 2009 and the feud between Paul Krugman and John Cochrane.

131 Although the Post Keynesian ideas of Hyman P. Minsky, in particular his Financial Instability Hypothesis, have attracted considerable renewed attention in macroeconomics.

132 See Zingales, 2012, for a far more elaborate exposition of his viewpoints.

Zingales is very critical on both the practice of finance and banking and the academic field in his confronting article titled "Presidential Address: Does finance benefit society?". While he basically is of the opinion that finance "fosters growth, promotes entrepreneurship, favors education, alleviates poverty and reduces inequality", defending finance as contributing to general welfare has lost credibility, he warns. Moreover, this loss of credibility has provoked not only legislation and regulation (well-intended but often misguided) but also even more lobbying from the financial sector with more undesirable consequences. Academic scholars have a role to play in preventing escalation of a vicious circle. Listing a host of excesses in the financial sector (mis-selling, manipulation, fraud), academics should be less agnostic and more critical about what benefits society and what does not. In addition, they should be more careful in advertendly or inadvertendly aligning themselves with other stakeholders in the financial sector: not only with businesses but also with regulators as well as policy-makers. Instead they have a duty to publicly voice and educate what is good and what is bad in terms of products, market structure, regulation, policy, etc., according to Zingales.

What could be other ways forward then for thinking and dealing with the phenomenon of financial markets? In chapter four it was argued that explanations from different schools of thought that may appear irreconcilable at first sight need not always be rival but can be complimentary if one examines the particular claims carefully enough. That leaves room for a more pluralistic approach. The crisis actually has spawned initiatives in that direction.

## 6.3 GEORGE SOROS: REFLEXIVITY AND THE INSTITUTE FOR NEW ECONOMIC THINKING

The crisis has brought attention to the ideas of investment billionaire, philanthropist and philosopher George Soros. Fueled by his rich experience in the markets and inspired by his LSE education with Karl Popper as his tutor, Soros (1997, 2013) has developed a framework which, he believes, not only applies to financial markets but also to the social sciences in general and economics in particular. The framework hinges on two concepts: fallibility and reflexivity:

> My conceptual framework is built on two relatively simple propositions. The first is that in situations that have thinking participants, the participants' views of the world never perfectly correspond to the actual state of affairs. People can gain knowledge of individual facts, but when it comes to formulating theories or forming an overall view, their perspective is bound to be either biased or inconsistent or both. That is the principle of fallibility (Soros, 2013).

> The second proposition is that these imperfect views can influence the situation to which they relate through the actions of the participants. For example, if investors believe that markets are efficient then that belief will change the way they invest, which in turn will change the nature of the markets in which they are participating (though not necessarily making them more efficient). That is the principle of reflexivity (ibid.).

Both concepts share familiarity with concepts and ideas which have been treated in earlier chapters, in particular chapters four and five. Soros explicitly espouses Frank Knight's distinction between calculable risk and (radical) uncertainty (see also Frydman & Goldberg, 2013). According to Soros (2013) "it is fallibility that is the key source of Knightian uncertainty in human affairs"[133].

Regarding reflexivity Soros distinguishes between a cognitive and a manipulative function in the thinking of agents. The cognitive function is a passive one, observing the world while the manipulative function is active, intervening in the world. When both function are active they interfere with each other and the outcome becomes sketchy:

> Consequently, the cognitive function cannot produce all the knowledge agents need to make decisions; they have to act on the basis of imperfect understanding. While the manipulative function can make an impact on the world, outcomes are unlikely to correspond to expectations. There is bound to be some slippage between intentions and actions, and further slippage between actions and outcomes. Since agents base their decisions on inadequate knowledge, their actions are liable to have unintended consequences. This means that reflexivity introduces an element of uncertainty both into the agents' view of the world and into the world in which they participate (Soros, 2013).

The way Soros looks at agency, based on fallibility and reflexivity and explicitly including Knightian uncertainty, clearly shares affinity with the picture of agency that was painted in chapter four, inspired by Austrian market process theory. Indeed several authors have noted similarities between the ideas of Soros and members of the Austrian school, in particular Hayek (Caldwell, 2014, Bronk, 2014).

But there are also links with the concept of performativity, as treated in chapter five. Soros (2013) writes that "in social systems fallible human beings are not merely scientific observers but also active participants in the system themselves. That is what makes social systems reflexive". Bronk (2013), referring to Soros (1997), comes to the conclusion that reflexivity amongst other things also implies that there is two-way interaction between science and real-

133 One could perhaps also argue that it is the other way around: Knightian uncertainty being a cause for fallibility.

ity. Put differently: there are feedback loops between science and reality. Which is precisely the point of the performativity thesis (see also Hands, 2014).

So far so good but what is the upshot of Soros' ideas? In his own words: "What makes my propositions interesting is that they contradict some of the basic tenets of economic theory" (2013). Soros thinks that reflexivity, in combination with fallibility, gives rise to self-reinforcing feedback loops which lead to disequilibrium. For Soros market prices do not accurately reflect fundamental value and agents' views of the world are never perfectly aligned. Equilibrium is the exception rather than the rule; Soros sees this standard economic assumption as an extreme and limiting case (Bronk 2013). Because neoclassical economics, in particular Rational Expectations and the Efficient Market Hypothesis, cannot deal with reflexivity, there is a need for a new economic paradigm, according to Soros.

Implicitly, and maybe fueled by the 2007-2008 crisis, Soros appears to have either the opinion that disequilibrium is undesirable or he shares some form of the radical subjectivist view of Lachmann and Schackle which were mentioned in chapter four. That is the idea that the amount of ignorance is so great that it puts coordination beyond reach, which renders the concept of equilibrium useless. Soros indeed appears to hold that fallibility is quite omnipresent and universal.

With regard to equilibrium in chapter four it was suggested to take a step back from it because it is somewhat of a disputable concept in the context of financial markets where supply and demand continuously clear. It was also suggested to decouple the notion of market efficiency from the concept of equilibrium, in the light of the dynamic market process which actually takes place in the market. In other words, disequilibrium need not be an insurmountable problem. Like myself, other authors, while being interested in the concept of reflexivity have also questioned Soros' conclusion about equilibrium and neoclassical economics. Guala (2014) agrees the importance of reflexivity, but think that his critique of mainstream economics is mistaken. Features of reflexivity can be built into neoclassical models. The challenge is practical, rather than conceptual. There is no deep flaw in economic theory that prevents it from capturing the essential features of "negative" and "positive feedback loops", he concludes. Bronk (2013) reaches a similar conclusion. He finds Soros' view of (nearly) universal fallibility similarly unconvincing as the rational utility maximizer from neoclassical economics. Bronk also takes issue with the stance he perceives in Soros that performative effects have a priori negative effects. He concludes that Soros should acknowledge that both neoclassical economics and his own theory capture certain important but partial truths about markets. I couldn't have put it better myself.

The perceived need for a new economic paradigm, led Soros to co-founding the Institute for New Economic Thinking (INET). The goal of INET is:

> Founded in the wake of the financial crisis in 2009, the Institute for New Economic Thinking (INET) is a nonpartisan, nonprofit organization devoted to developing and sharing the ideas that can repair our broken economy and create a more equal, prosperous, and just society.

The advisory and governing boards are composed of a blend of economists and other social scientists, bankers, investment managers and journalists.

The idea behind INET is interesting and the reasons for its inception are valid. However, the assumptions underlying the idea do not appear to be free of preconceptions:

> We have seen all too clearly how free market fundamentalism, fiscal austerity, financialization and corporate influence in politics have endangered economies, communities, and the planet as a whole.
>
> Left to their own devices, academic, governmental, and corporate institutions will continue to cling to outmoded economic models, out of fear that new ideas would undermine their own financial advantage.
>
> We work to guide the field away from economic orthodoxy so that it can free itself of inertia and past failures.

Mentioned as one of the key principles is that heterodox models that pose alternatives to the neoclassical orthodoxy are essential to understanding the economy and promoting a vibrant intellectual pluralism[134]. As such it would appear that INET is more of a thinktank which advocates heterodoxy rather than a champion of pluralism. Where it concerns the value of alternative explanations, be it orthodox or heterodox, the proof of the pudding is in the eating. In chapter three this was somewhat tested for behavioral and neoclassical finance. And in chapter five it was shown that the sociological angle is valuable, but not so much as to fully discredit the orthodoxy. When a plurality of theories applies to a subject matter, there is no reason why orthodoxy should or could not be one of those. In addition, as Davis (2012) has pointed out, the orthodoxy in economics is not necessary static:

134 Other key principles mentioned are that economists and their ideas should be independent from powerful interests, attention for complexity and uncertainty, inequality and distribution, history, diversity, multidisciplinary learning.

> This core-periphery framework, however, is a dynamic one, and as the history of economic thought demonstrates what counts as core and periphery research programs has changed over the history of the discipline, thus implying that the identity of the discipline as associated with its core research programs also changes over time.

That applies to finance as well, I believe. As I argued in chapter two, finance has changed and evolved dramatically over time. The core being qualitative, institutionalist just after the war. The development of the major finance theories, which can be considered as originally work in the periphery, gave rise to a new formalistic quantitative core. And behavioral finance is a prime example of research development in the periphery culminating in becoming part of the core.

## 6.4 CRITICAL FINANCE REVIEW

In chapter two the methodology of finance was analysed by means of categorizing subject and approach in the two leading finance journals: the Journal of Finance and the Journal of Financial Economics. One of the findings was the dominance of empirical contributions, in particular statistical data analysis. However, it was noted that since the crisis there appeared to be some signs of more variety within the empirical work. An interesting development in this regard has been the inception in 2012 of a new journal: the Critical Finance Review (CFR). Founder and editor-in chief, UCLA finance scholar Ivo Welch, wanted to create a journal unlike others: with room for controversy and critique (Welch, 2012). In the CFR typically some well-known, established part of knowledge is critically reviewed while the original authors get the opportunity to reply in that same issue. Another form of critique is that in the CFR regularly replication checks of older papers are done to see if the results still hold up.

In addition, Welch wants to create space for less usual but important topics, such as social issues, climate change and demographics. Examples are "The Housing Wealth Effect: The Crucial Roles of Demographics, Wealth Distribution and Wealth Shares" (Charles Calomiris, Longhofer and Miles, 2013) and "Obesity and Household Financial Distress" (Guthrie and Sokolovsky, 2017).

While the CFR is still a very young journal, it is already well-regarded within the field and it has a top ten position amongst finance journals where it concerns impact. Top-level scholars, such as Fama and French, Shiller, Campbell, Lo, Roll and many others have written contributions. The approach in the CFR remains deeply empirical, like it is in the Journal

of Finance and the Journal of Financial Economics[135]. Welch himself has said that one of his goals is to change the incentives for finance writers. By creating his new outlet he wants to offer an alternative for the established journals in which scholars do not have to follow the traditional mould of the particular established journal. But we could also speak of an attempt to change and add to the conversation in finance. The conversation is changed because the critique and reply model (a conversation in itself) provides a much more direct dialogue than can be accomplished by the traditional leading journals with lengthy processes and time periods to publish. And the conversation can be augmented if scholars take the opportunity to publish about less usual subjects which, in the words of Welch (2012), are critical in the meaning of importance.

## 6.5 INSTITUTIONALISM IN FINANCE

In chapter two the transition from finance as a descriptive, non-mathematic endeavor to a quantitative, formalistic discipline was described. The former being labelled "institutional finance". In his 2007 book Peter Bernstein (2007) identified a group of scholars within the orthodoxy of finance whom he labels "institutionalists". By using the phrase "institutionalist" he meant a focus on the end-users of financial markets: firms seeking capital, pension funds, insurers, social security agencies and individuals who need to manage their consumption and savings over time. In the context of financial markets I would add that an institutional perspective also entails dealing with a variety of aspects which impinge on the functioning of financial markets. Financial markets do not exist just for the sake of playing the game of making and losing money. They do exist to deal with the problem of inter-temporal consumption decisions, or in other words: to cope with time, risk, and uncertainty. In a financial marketplace which is increasingly globally connected and high-tech this ultimate raison d'état should not be forgotten.

For markets to function properly, the institutional structure needs to be right, either by design or by evolution. To repeat Rajan and Zingales (2003: "markets cannot flourish without the very visible hand of the government, which is needed to set up and maintain the infrastructure". In other words, unregulated markets are by no means always preferable to regulated markets, and vice versa. Rajan, Zingales and others with a Chicago School background have also always been quite critical on lobbying and other efforts by special interests to influence policy makers and thus alter the competitive environment. They share this viewpoint with institutionalists like J.K. Galbraith. The extensive and successful lobbying by

135 Only a limited number of issues has been published so far. An analysis, as was performed in chapter 2 for the Journal of Finance and the Journal of Financial Economics, would be a bit premature.

the financial sector for deregulation in the years leading up to the crisis is well-documented and mentioned as one of the underlying causes for the 2007-2008 crisis.

While regulation is an obvious part of the story, the Austrian perspective on the market process, treated in chapter four, can be regarded as institutional as well. To acknowledge that institutions matter, implies that the well-functioning of capital markets is not solely a matter of letting market forces run wild. The interests of the end-users of financial markets need to be taken into account and safeguarded. This may be done in a number of ways: regulation obviously springs to mind, but we can also think about, for instance, education and the creation of stimuli to use the full spectrum of products that financial markets have to offer.

Agency theory, which can be regarded as partly neoclassical, partly (new) institutional economics, is also relevant, most notably where it concerns the problem of moral hazard. During the crisis failing banks were bailed out by central banks and governments for the sake of stability of the financial system and the economy in general, effectively creating a put option under the activities of those banks, which was largely ultimately paid for with the taxpayer's money. No banker will admit that they counted on the guarantees that governments provided at the peak of the crisis to bail them out. Or that they advertently or inadvertently took on more risk than they would have without counting on some kind of safety net. But many of them were bailed out in a collaboration of the sector itself with government and regulators with the taxpayers largely footing the bill. Another consequence of the crisis and bail-out was a reshuffle of the sector where a few dominant market participants got even more dominant, thus reducing competition in the market and perhaps increasing lobbying strength.

There has also been a lot of attention for the compensation of executives: performance-related compensation can misalign the interests of executives and shareholders (and other stakeholders). Finally, in the context of financial markets a similar problem within organizations pops up: payment of professional traders and money managers is also highly performance-linked and usually based on a short time horizon. In all these cases incentives are present to take on extra risk, which might not be prudent. These are all agency-related issues.

According to Bernstein (2007) finance scholars such as Robert Shiller, Andrew Lo and Robert Merton belong to the institutionalist category. The common denominator of their work is that they take financial markets beyond the narrow realm of the markets themselves to real-world problems such as pensions and retirement and housing. Shiller, for instance, has occupied himself with the housing market in the 2005 version of his bestseller "Irrational Exuberance". This has led to construction of the Case-Shiller index: an indicator for the housing market which permits people to keep track of the housing market and potentially

deal with the risks and uncertainties that are presented in that context. In his 2012 book "Finance and the Good Society" Shiller expands his argumentation further to use finance and financial markets for the good of society with regard to insurance, pensions, college savings, and so forth.

Likewise Merton, with Fischer Black and Myron Scholes one of the originators of option pricing theory, has occupied himself with lifecycle finance and products and markets that relate to pensions and retirement (Merton and Bodie, 2005). Financial innovation for Merton means better solutions for the end-users of financial markets. Merton and Bodie point out that transaction costs are extremely relevant to actual financial markets and products. That is another typical institutional notion, expressing the idea that the existence of these markets and products hinges on trying to minimize transaction costs.

Lo is concerned with a variety of issues that surround financial markets, such as market structure, regulation and risk management. One could add the late Stephen Ross who consulted on smart compensation schemes for executives, based on his expertise in agency theory and options. What is interesting is that the reputation of these scholars originates in their work in the narrow realm of financial markets: market efficiency and asset pricing. Also note that the "institutionalist" label runs across the behavioral-neoclassical demarcation: Ross was a champion of neoclassical finance while Shiller is considered a premier behavioral/Keynesian economist in the first place.

## 6.6 NEW INSTITUTIONAL FINANCE

If we accept that a plurality of partial explanations applies to financial markets, rather than one grand theory, some kind of umbrella concept might be useful. Perhaps we could speak of new institutional finance.

The obvious similarity of this phrase with new institutional economics is no coincidence. In his 2000 discussion article on new institutional economics Oliver Williamson recommends that, by lack of or awaiting a unified grand theory, we should accept pluralism. Furthermore, Williamson situates new institutional economics closer to the orthodoxy than old institutional economics. New institutional economics adds to the orthodoxy rather than replacing it. That corresponds to the ideas which have been suggested in the previous chapters, in particular in chapter four: there can be room for other explanations next to the (neoclassical) hardcore.

Finance remains essentially and above all a social science. Williamson (2000) identifies four levels of social analysis:

- Level 1: informal institutions , such as customs, traditions, norms, religion, ethics;
- Level 2: formal institutions, such as property rights and other macro legal and political arrangements;
- Level 3: governance, such as firms and other forms of organization;
- Level 4: resource allocation, e.g. the market.

Higher levels pose constraints on the lower levels, while there is also feedback from lower levels to higher levels. Perhaps the spirit and inspiration of Williamson's scheme (though certainly not identical) can be useful for framing a new institutional finance. The emergence, and existence of financial markets and the basic functions these markets perform can be regarded as a level 1 issues. Level 2 is about market structure. It is constituted by the conditions which affect and influence the well-functioning of financial markets: legitimacy, transparency, liquidity and the according legal boundaries and relevant policy-making, governance and supervision. Level 3 is the market itself, primarily but not exclusively the domain of neoclassical finance. The behavioral and psychological angle constitutes level 4: the micro motives of individual agents. The Austrian market process theory is an example of a feedback loop, in this case between level 4 to level 3. Reflexive and performative aspects can play on all levels.

## 6.7 FINALLY

I have argued that it can be worthwhile to employ various perspectives when studying and analyzing phenomena in financial markets: social-institutional, economical, psychological, and so forth. Within the economic frame neoclassical finance is the dominant approach, though not the only one. As has been shown finance has pluralistic characteristics. It has evolved from a field with a descriptive, qualitative approach into a quantitative discipline that emphasizes rigor and (mostly statistical) testing. Finance became one of the poster boys of neoclassical economics, a testing ground for its methods and theories. But it would be well served to not forget about those pluralistic origins and keep an open stance to novel approaches. Even while the neoclassical core still forms a solid fundament, new ideas may be profitable. Not in the monetary sense, because making money in the markets is not easy, but there can be value in, for instance, an enhanced picture of agency in financial markets (using behavioral insights amongst others), in descriptive accounts of how arbitrage works (using Austrian market processes and other approaches emphasizing discovery, learning, evolution and adaptation). And there may be much more to gain in a more heterodox finance using approaches such as bubble finance, fractal finance, political finance, neurofinance ,

Keynesian inspired finance, as well as innovations from the practice. Most of these ideas can be empirically tested. Financial markets have been the proving ground for a number of neoclassical concepts (Harrison, 1997) but they can function in a similar way for other approaches, as has happened already for behavioral economics.

Earlier the possibilities of reconciliation of seemingly rival economic schools of thought and disciplines, on a methodological level  but perhaps also on an ideological level, were discussed (see also Davis, 2019a). I suggested that the differences and disagreements, polarized as they may appear at first sight, could actually be bridged to some extent, at least in finance. The result is a realist-grounded pluralism: a plurality of theories (and methods) which deliver partial and approximate explanations (see Mäki, 2005, Marchionni, 2005). These explanations are not universal, but work local and contextual, on particular aspects of and in specific cases within the broader phenomenon of financial markets.

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## SUMMARY

The aim of this dissertation is twofold:
1) To add to a meaningful conversation on financial markets by using less conventional, yet empirical, methods, which differ from the dominant statistical empirical methods;
2) To provide a case study of what methodological pluralism can look like: applying various methods and perspectives to one and the same subject or phenomenon.

While the various chapters may appear somewhat unrelated, the thread throughout is the interplay of theory and practice in financial markets.

In chapter 1 the key concepts are introduced: the basics of financial markets and what role they have in the social-economic realm. Various notions of pluralism are discussed as well.

The second chapter contains an empirical investigation of the methodology of finance. An extensive sample, covering the entire history of the two leading journals in the field, the Journal of Finance and the Journal of Financial Economics, has been investigated in order to sketch:
1) The development of subjects of research;
2) The approach used in tackling those subjects.

Important changes through time were found and possible explanations for these changes are provided. The major findings are:
1) A shift in the subjects of the writings over time: a narrowing of the scope of analysis from broader (macro-)economic policy-oriented issues, first towards analysis of market and firm behavior and later increasingly towards behavior of agents and singular events;
2) Two major changes in approach from chiefly empirical qualitative research first towards predominantly theoretical analysis and subsequently back to mainly empirical, but now quantitative-statistical, research.

Both appear to be facilitated and fueled by:
1) The development of ground-breaking theory in the 1960s and 1970s, which in turn is part a general wave of formalization and quantification in economics in the 1950s and 1960s;
2) The technological advances of the middle 1980s that propelled the rise of ever more precise and detailed empirical data analysis.

The third chapter explores one of the main divides in thinking about financial markets, between neoclassical and behavioral theories: do financial markets behave in a more or less

efficient manner, which would imply that excess returns are unachievable in the long run, or are there systematic and enduring deviations which can be exploited to provide superior returns? Some of the leading protagonists on both sides of this debate happen to be involved in professional asset management operations. The real-world performance of these operations is compared in terms of risk and return in order to provide a new perspective on the neoclassical versus behavioral debate in finance. While the sample is small, the upshot is twofold. First, there is no conclusive evidence that investment management operations with strong academic ties consistently and persistently produce better than average returns. No more and no less, when compared to other professional mutual funds, at least based on the funds analyzed here. Second, there is no conclusive evidence that investment management operations which are advertised as based on principles of behavioral finance do provide consistently and persistently superior returns when compared to investment management operations which are advertised as inspired by principles of neoclassical finance, or the other way around for that matter.

In the fourth chapter ideas out of Austrian economics, in particular Israel Kirzner's thoughts on the market process, are applied to the workings of, and theorizing about financial markets. I argue, both theoretically and by using illustrations out of the practice of financial markets, that a descriptive causal process approach such as the Austrian viewpoint provides, can be regarded as complementary to the more normative claims put forward by neoclassical and behavioral finance, actually capable of bridging the divide between these two dominant strands of thought in finance.

Performativity is a concept that relates to the idea that theory and practice interact and influence each other, a theme which runs throughout this dissertation. Performativity can be described as the idea that a theory or an aspect of a theory such as a model in some form enacts the reality it is intended to describe. Chapter five scrutinizes the arguments brought forward on the performativity of finance theory, in particular as presented by Donald MacKenzie. It will be argued that, while MacKenzie has made a valuable and meaningful contribution, his strongest claims—that option pricing theory shows traits of what he calls Barnesian performativity—do not hold up. This is due to a faulty reading of the Black-Scholes-Merton model and option pricing theory in general and a questionable appreciation of the nature of economic phenomena and models and theories used in economics. It is argued, however, that the less extreme version, so-called effective performativity is a sensible and interesting concept,.

This research project first began to take shape in 2005, i.e. before the 2007-2008 crisis broke loose. Since 2007 financial markets and the thinking about financial markets have profoundly changed. Some aspects related to the crisis are treated in the final chapter as well

as some new interesting developments within finance which can be regarded as offspring of the crisis. Subsequently suggestions are made which could improve the conversation on financial markets. An attempt is made to sketch the outlines of what a pluralistic finance would look like, which I have labelled new institutional finance.

## SAMENVATTING

Het doel van deze dissertatie is tweeledig:
1) Een bijdrage leveren aan de academische conversatie over financiële markten door gebruik te maken van minder conventionele empirische methoden die afwijken van de traditionele statistische aanpak;
2) Een illustratie geven van methodologisch pluralisme: het toepassen van verschillende methoden en perspectieven op een en hetzelfde onderwerp of fenomeen.

Hoewel de verschillende hoofdstukken op het eerste gezicht losstaand van elkaar mogen lijken, is de rode draad de interactie van de theorieën over en de praktijk van financiële markten.

In het eerste hoofdstuk worden de kernconcepten geïntroduceerd: wat financiële markten eigenlijk zijn en welke functie ze vervullen binnen de maatschappij. Daarnaast worden verschillende betekenissen van het begrip pluralisme besproken.

Hoofdstuk twee bevat een empirisch onderzoek naar de gebruikte methodologie binnen het vakgebied financiële economie. Een uitgebreide steekproef van de twee leidende wetenschappelijke publicaties binnen het vakgebied financiële economie, Journal of Finance en Journal of Financial Economics, is onderzocht op twee aspecten:
1) De ontwikkeling van de onderwerpen van onderzoek;
2) De gebruikte benadering om die onderwerpen te onderzoeken.

Belangwekkende veranderingen in de tijd worden zichtbaar waarvoor verklaringen zijn. De belangrijkste bevindingen zijn:
1) Een verschuiving in de behandelde onderwerpen in de vorm van een vernauwing van de reikwijdte van de analyse van brede (macro-)economische beleidsgeoriënteerde kwesties naar eerst analyse op het niveau van markten en organisaties en vervolgens onderzoek naar individuele agenten en gebeurtenissen;
2) Twee majeure veranderingen in de aanpak en methoden: van eerst voornamelijk kwalitatief empirisch onderzoek naar hoofdzakelijk theoretische analyse en vervolgens weer terug naar empirisch, maar nu kwantitatief-statistisch, onderzoek.

Deze ontwikkelingen lijken gefaciliteerd en gevoed te zijn door:
1) De ontwikkeling van baanbrekende theorieën in de Jaren zestig en zeventig van de vorige eeuw, welke op zijn beurt weer onderdeel is van een bredere golf van formalisering en kwantificering binnen de economie vanaf de jaren vijftig en zestig.

2) De technologische vooruitgang sinds de jaren tachtig, die steeds preciezer en gedetailleerdere empirische data-analyse mogelijk maakte.

Hoofdstuk drie gaat in op een van de belangrijkste meningsverschillen in het denken over financiële markten, namelijk die tussen de neoklassieke school en de gedragseconomen. Het meningsverschil draait om markefficiëntie: gedragen financiële markten zich op een min of meer efficiënte wijze zodat het op lange termijn niet mogelijk is om de markt te verslaan of zijn er systematische en aanhoudende afwijkingen waardoor dat wel degelijk mogelijk? Enkele van de academische hoofdrolspelers zijn ook betrokken bij professionele vermogensbeheeractiviteiten en de prestaties van die activiteiten in termen van risico en rendement zijn met elkaar vergeleken. Hoewel de steekproef klein is, kunnen er wel twee conclusies worden getrokken. Ten eerste is er geen doorslaggevend bewijs te vinden in de data dat beleggingsfondsen met sterke academische banden bovengemiddeld presteren in vergelijking met andere professionele fondsen. Ten tweede is er geen doorslaggevend bewijs dat gedragseconomisch geïnspireerde fondsen beter presteren dan neoklassiek geïnspireerde fondsen. Andersom geldt overigens hetzelfde.

In het vierde hoofdstuk worden ideeën uit de Oostenrijkse school, in het bijzonder het gedachtengoed van Israel Kirzner over het marktproces, toegepast op de theorie en praktijk van financiële markten. Ik beargumenteer theoretisch en geïllustreerd door praktijkvoorbeelden dat een beschrijvende causale procesbenadering, zoals Kirzner die geeft, gezien kan worden als complementair aan de meer normatieve claims, zoals die door de neoklassieke economie en gedragseconomie te berde worden gebracht aangaande financiële markten. Ergo, dat deze Oostenrijks benadering een brug kan slaan tussen deze twee dominante gedachtestromen.

Performativiteit is een concept dat gerelateerd is aan het idee dat theorie en praktijk interactie hebben en elkaar beïnvloeden. Het kan beschreven worden als het idee dat een theorie (of een aspect van een theorie zoals een model) op enigerlei wijze de realiteit beïnvloedt waar op die theorie betrekking heeft. Hoofdstuk vijf onderzoekt de argumentatie zoals die door Donald MacKenzie naar voren is gebracht met betrekking tot de performativiteit van financiële economie. Ik beargumenteer dat, hoewel de ideeën van MacKenzie nuttig en waardevol zijn, zijn meest vergaande claim dat optietheorie zeer sterk ("Barnesian") performatief is, te ver gaat. De redenen daarvoor zijn dat MacKenzie optietheorie in zijn algemeenheid en het Black-Scholes-Merton model in het bijzonder onjuist interpreteert. In zijn algemeenheid is zijn perceptie van economische fenomenen en de aard van theorieën en modellen die binnen de economische wetenschap gebruikt worden betwistbaar. Desalniettemin is zijn idee van performativiteit van financiële economie in een minder vergaande variant ("effective") belangwekkend en interessant.

Dit onderzoeksproject is gestart in 2005, dus voordat de grote financiële crisis van 2007-2008 uitbrak. Sindsdien zijn zowel de financiële markten zelf als de gedachtenvorming daarover ingrijpend veranderd. In het slothoofdstuk worden enige crisis-gerelateerde aspecten behandeld evenals nieuwe ontwikkelingen die gezien kunnen worden als het product van die crisis. Tenslotte doe ik suggesties hoe de conversatie over financiële markten verbeterd kan worden en probeer ik te schetsen hoe een pluralistische financiële economie, die ik nieuwe institutionele financiële economie ("new institutional finance") heb genoemd, eruit zou kunnen zien.

## ABOUT THE AUTHOR

Job Daemen was born in 1970 in Beuningen, the Netherlands. In 1994 he obtained an MSc ("drs.") in business economics/finance from the University of Groningen. Between 1995 and 2003 he worked in the financial markets as a trader and manager in the Netherlands and the United States. In 2004 he obtained an MPhil from the Erasmus Institute for Philosophy and Economics, Erasmus University Rotterdam, after which he started his PhD. In the past ten years he has worked predominantly as a researcher and adviser in large policy research projects. Job currently lives in Amsterdam with his wife and five children.